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The Concept of Probability

A Critical Survey of Recent Contributions

BY

CHARLES E. BURES

INTRODUCTION

IT IS the purpose of this paper to review the present status of discussion concerning the concept of probability. The exposition will consist of six sections: (1) A brief statement of the most important traditional views of the concept of probability: subjective and objective; (2) A brief criticism of two traditionally important conceptions: a priori probability and the Principle of Indifference; (3) A criticism of an outstanding example of the subjective theory of probability, the theory of F. P. Ramsey; (4) A criticism of the most widely known of the objective logical theories of probability, the theory of J. M. Keynes; (5) Some remarks concerning the results of the recent work of Reichenbach and von Mises with particular reference to clarification of the concept of probability; (6) A summary of the case for the frequency interpretation of probability, revealing its basic importance in the light of present evidence.

TRADITIONAL VIEWS OF THE CONCEPT OF PROBABILITY

The most important interpretations that have been given to the concept of probability may be conveniently divided into two major subdivisions: subjective and objective interpretations. The objective group divides further into logical and statistical concepts of probability.

Historically the incompleteness of knowledge and the resulting ignorance as to the probable occurrence of events led to a belief in the

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subjectivity of the concept of probability. This belief was augmented by the growing conviction that probability is a relation, and relations were regarded as necessarily subjective and mental. On this theory the concept of probability gained meaning variously as a psychological degree of certainty, belief, expectancy or conjecture. In general, application of the calculus of probabilities to these subjective references was effected through the use of the classical definition of probability as the ratio of the number of favorable alternatives to the total number of equally probable alternatives.

The manifest difficulties of establishing a theory of probability on a subjective basis turned the attention of theorists to a search for more objective standards in logical theories. Keynes has been for many years the outstanding exponent of the interpretation of probability as a logical relation between propositions. Further he held that objectivity for a logic of probability is attainable only if this probability relation is immediately intuitive. The theory of Keynes, as representative of this standpoint, will be criticized in more detail later in this paper.

With the rise of statistical applications of increasing complexity and importance not only in the social sciences but also in physics, the interpretation of probability in terms of relative frequency has gained increasing prominence. Historically the frequency theory first appeared in a short essay by Leslie Ellis (1842). At practically the same time the notion appeared in the work of Cournot (1843). But the first attempt at systematic discussion was made by Venn. By relative or statistical frequency is meant the ratio of the number of appearances of a definite event to the total number of a class or aggregate of events of which the occurrences of the event in question form a subclass. Implicit in these early formulations was the later refinement of probability defined as the limit of the relative frequency, involving the assumption of an infinite series of events. Fundamental to this latter interpretation is the notion of the defined aggregate of events in which the frequency ratio is established. It is generally granted without argument that in vital statistics and in physics the frequency interpretation of the concept of probability is the only adequate one. Concerning other applications, agreement is hardly unanimous. It will be the aim of this paper to show that underlying all of the various interpretations there is a basic frequency interpretation, tacit or explicit.

This meaning of the concept of probability retains the valid contributions of the subjective and objective logical theories and adds some additional important factors. On this theory probability propositions

become material propositions capable of empirical test (confirmation in Carnap's sense).[1] Perhaps of most striking importance is the fact that probability as given by the frequency interpretation is a proper fraction, which yields directly the numerical probability, given adequate statistical data. It was on the problem of numerical application that earlier theories met insurmountable conceptual difficulties.

A PRIORI PROBABILITY AND THE PRINCIPLE OF INDIFFERENCE

Because of their widespread influence, two other notions traditionally associated with probability theory deserve careful consideration, namely, the concept of a priori probability and the Principle of Indifference (or Principle of Insufficient Reason).

It is a truism that probability theory originated historically in problems of gambling. Now gambling mechanisms have predominant structural symmetries, giving to the coördinate terms of the probability situation (events) the character of a complete and exclusive disjunction. Basing determinations of probability degrees on these structural peculiarities led quite naturally to the judgment that the terms of the disjunction are equally probable and that all situations admitting the application of probability theory are reducible to a basic symmetry. The conviction that the judgment of equal probability can be made independently of statistical experience, that is, by reasoning alone, is the meaning of the concept of a priori probability. The determination of probability degrees follows immediately. If the disjunction is n-termed, the probability of occurrence of each of the terms is $1/n$, using the classical definition of probability.

Evidently the validity of this theory in application rests on the validity of the concept of 'equal probability', usually considered to be logically inferable from the basic symmetry of the material conditions. In making the judgments of equal probability, theories built on this foundation resort to a Principle of Indifference. Stated summarily, such a principle asserts that if of n (finite) alternatives there is no reason for judging any one alternative more probable than any other, the n terms are equally probable and each has the probability $1/n$. Application of such a principle clearly requires a basic symmetry with a complete and exclusive disjunction.

Acceptance of this facile symmetry of game-of-chance mechanisms as a basis for the concept of probability necessitates that many far more

[1] Carnap, R., "Testability and Meaning," *Philosophy of Science*, October, 1936.

complex situations must be reducible to a basic symmetry if the theory of probability is to be widely and fruitfully applicable. In a few simple cases this can be done with impunity, but in general this reduction cannot be made. If it can, the problem of validity of the equal probabilities still remains.

The difficulty is that no logical connection exists between the geometrical and mechanical symmetry of a die, for example, and the frequency of occurrence of a side. No chain of logical reasoning permits the conclusion that an improbable run of events gives any reason for suspecting the symmetry of the mechanism. Observed frequencies with dice underlie our judgments of indifference and of equal probability. When one considers that the perfectly symmetrical die with respect to construction, initial position, marking, coloring, and the like, is only a theoretical ideal, then it is clear that irrelevance can only be known a posteriori. Lacking a complete disjunction, the Principle of Indifference is consequently inadequate and inapplicable, unless the various factors have been judged irrelevant on other grounds, namely, a definite body of empirical knowledge. Determinations of probability are a priori only in the sense of independence of specific statistical experience, not independence of all statistical experience.

The meaning of the concept of probability in science and in everyday life is not sufficiently covered by any theory founded on a Principle of Indifference. All of the conclusions of apriorism in probability are obtainable by means of purely a posteriori knowledge and symmetry based on physical measurements. Investigation of game-of-chance mechanisms has shed much light on certain general probability properties of natural phenomena as they appear in the special form of equal probable cases, but these special cases cannot, therefore, be taken as logically primitive in probability theory. In summary, a priori determination of degrees of probability reduces to a posteriori computation of statistical frequencies or at least to a rough evaluation of such frequencies.

THE SUBJECTIVE THEORY OF F. P. RAMSEY

Following this condensed summary of a few fundamental conceptions of probability theory, it should prove fruitful to examine and criticize some specific theories representative of traditional interpretations of the concept of probability. The subjective view will be considered initially.

Undeniably individuals do possess a psychological feeling of expectancy; also this feeling seems to show varying degrees of intensity which

may serve as a basis for degrees of probability. But the presence of desires, hopes, fears, wishes and various other affective states makes this psychological feeling much too variable and vague to serve as a reliable foundation for the probability concept. The most valid aspect of this subjective factor is a propensity for being more or less accurately determined on the basis of long experience of statistical frequencies. The 'worldly wise' individual 'expects' with reasonable accuracy. The weather prophecies of the veteran seaman, whose fortunes depend in such great part on weather phenomena, are proverbially accurate. A more detailed study of a specific attempt to establish a theory on a subjective basis will reveal more clearly some of the difficulties which must be met.

Perhaps no better choice for an exemplification of the subjective interpretation of probability could be made than the theory of a brilliant opponent of the frequency interpretation, the late F. P. Ramsey. He had great respect for the frequency theory of probability and made many admissions in favor of it, but he did not himself believe that probability as used in science is really the same as statistical frequency. Being an accomplished logician, Ramsey also opposed the frequency interpretation because of a difficulty in its logical construction, the so-called convergence problem to be discussed in a later section. Ramsey felt that there is a great deal of authority and evidence for interpreting probability as the logic of partial belief, and he attempted to systematize such a theory. It is of manifest importance to a supporter of the frequency theory to investigate carefully the validity of Ramsey's contribution, for his attempt is perhaps the most elaborate and cogent effort to establish a subjective theory.

Before giving a short exposition of Ramsey's theory, it is interesting and pertinent to notice in his own words a statement which expresses in part the position taken in this paper. "It may be that, as some supporters of the frequency theory have maintained, the logic of partial belief will be found in the end to be merely the study of frequencies, either because partial belief is definable as, or by reference to, some sort of frequency, or because it can only be the subject of logical treatment when it is grounded on experienced frequencies."[2]

A theory of probability as a logic of partial belief demands a purely psychological method of measuring belief, according to Ramsey. From the measurement of belief there will follow by prescribed rules of

[2] Page 159, Ramsey, Frank Plumpton, *The Foundations of Mathematics* and Other Logical Essays, New York, 1931.

quantification, the measurement of probability degrees. It is evident that the whole theory succumbs, if beliefs are not measurable in some reasonably accurate manner. Ramsey contends that there are two features of the measurability of belief which must be carefully noted: (1) some beliefs are more accurately measurable than others; (2) since varying the method of measurement will vary the result, the method must be clearly prescribed. The analogy is advanced between degree of belief and the time interval between two events in relativity physics, the latter requiring exact statement of the mode of measurement, that is, the reference system.

Measurement of belief implies the assignation to any belief of a magnitude, thereby placing it in an ordered sequence of magnitudes. In addition to this topological ordering of beliefs, we must also be able to assign numerical values intelligibly to degrees of belief. The latter problem is obviously very important, if the theory is to calculate numerical probabilities.

Ramsey next considers two possibilities as the basis for his logic of partial belief. The first possibility, the intensity of belief or the feeling of conviction, he discards as inadequate with these words: "... the beliefs which we hold the most strongly are often accompanied by practically no feeling at all; no one feels strongly about things he takes for granted."[3] This reasoning seems patently false, when one considers that in any situation which threatens to alter such beliefs, plenty of feeling will be manifested. A more cogent position against intensity of belief has been advanced in the earlier discussion of the feeling of expectancy, namely, the variability due to other subjective factors.

The second possibility is the degree of belief as a causal property of the belief, or roughly "the extent to which we are prepared to act on it." Dispositional beliefs, that is, action under suitable circumstances, may be included for generality. Vague knowledge (or even utter ignorance) of causes of belief practically requires us to consider instead the differences between effects of degrees of belief. Thus probability as partial belief is concerned with the "measurement of belief *qua* basis of action."[4]

So Ramsey's theory is founded on a refinement of the old theory of measuring belief by offering a bet and securing the lowest possible odds. Subjective factors make this method too general and it must be reinforced through refinements. To gain generality and exactness, Ramsey resorts as a first approximation to the now discredited psychological

[3] Page 169, Ibid.
[4] Page 171, Ibid., Italics in the original.

theory that we act in the way most likely to gain for us our desired objectives.

After preliminary discursive attempts at definition, the theory is set up as follows. As a first assumption, the subject has certain beliefs about everything. Hence, by offering him options, he would be able to place all possible courses of the world in an order of merit. The problem still remains to assign numbers to these possible worlds. A second assumption is thereby necessitated, permitting the subject to doubt. Now it is possible to test degrees of belief in different propositions p by offering such options as this: Would you rather have world A in any event; or world B if p is true, and world C if p is false? If the truth of p were certain to the subject, his choice would follow a simple comparison of A and B. Doubts about p would naturally complicate the choice. Ramsey now states the aim of his theory: "I propose to lay down axioms and definitions concerning the principles governing choices of this kind."[5]

To circumvent possible subjective factors with respect to proposition p, the assumption is made that there are ethically neutral propositions. An atomic proposition p is ethically neutral, if two possible worlds, which differ only in regard to the truth of p, are always of equal value. A logical combination of ethically neutral propositions is ethically neutral. The system is now ready for the definition of numerical degrees of probability.

A subject has a degree of belief $\frac{1}{2}$ in an ethically neutral proposition p if he has no preference between the following options: (1) A if p is true, B if p is false, and (2) A if p is false, B if p is true, but has a preference simply between A and B. This is extended by an axiom to all pairs. As Ramsey then remarks, this definition of belief of degree $\frac{1}{2}$ is such as would lead to indifference between betting one way and betting another for the same stakes. This definition is generalized to form the basis for measurement of values by supposing that if world A is preferable to B, any world with the same value (that is, equal preference) as A is preferable to any world with the same value as B. The value of A is said to be greater than that of B, a relation which gives serial order to values.

After an axiomatic development of his theory, Ramsey concludes, and rightly so, that he has proved these laws of probability to be true necessarily for any consistent set of beliefs. These laws of probability

[5] Page 177, Ibid.

form an extension to partial belief of the laws of formal logic, the logic of consistency.

In any attempt to analyze the meaning of a concept it is necessary to ask how the concept is used in practice. Of interest in this paper is the difficulty in Ramsey's theory which arises when he attempts to apply it to the partial belief of human subjects. It is granted that if partial belief is to be consistent it must follow Ramsey's laws (granting their correctness). How does Ramsey propose to make the application of the calculus of probabilities which he has built to the measurement of degrees of probability as partial belief? From the words 'preference' and 'indifference' which appear in his fundamental definitions, it is clear that Ramsey resorts finally to a Principle of Indifference. The primitive step in assigning numerical values, the definition of degree of belief $\frac{1}{2}$ in an ethically neutral proposition, is based on lack of preference or on indifference between betting one way or another for the same stakes. Therefore, in accordance with the previous discussion of the Principle of Indifference, there is presupposed a tacit judgment whereby the subject satisfies himself that the two courses of action really permit of no preference and can safely be a matter of indifference. On the basis of what evidence are such suppressed judgments made? The only answer can be that they are made on the basis of experienced statistical frequencies, if they are made intelligently.

Ramsey remarks that his theory is based on betting and that this is not unreasonable in the light of the common belief that all life consists of betting. For example, whenever we go to a station to take a train, we are in a sense betting that there will be a train running at that time and place. But the whole point of criticism here is that we must have some experience of statistical frequencies as to the reliability of train schedules, or we should not be able to assess the bet we are required to make in such a situation. It is only on this basis, or on the basis of observed frequencies of other individuals, that judgments of indifference or of preference can be made in a trustworthy manner in science or in daily life. Subjective interpretations of probability are always forced to meet the problem of measurement of degrees of belief, expectancy or conjecture by turning to some form of the Principle of Indifference.

A further objection which may be raised is that propositions concerning subjective probabilities have no verifiable consequences. No possible prediction can be justified as a logical consequence of such propositions. In addition to these difficulties all subjective theories are unsuccessful in accounting for statistical phenomena.

THE OBJECTIVE LOGICAL THEORY OF J. M. KEYNES

Turning now to the consideration of a representative objective logical theory, the theory of Keynes affords the best example.[6] On this theory any two propositions have certain formal relations studied by logic, but in addition there is another directly intuitive relation called probability. This type of probable inference permits any rational being to proceed from full belief in a given proposition to partial belief in a second proposition. Any rational being under the same conditions must entertain the same partial belief in the second proposition on the basis of the first proposition. Furthermore, this probability relation is unanalyzable. Yet one must be able to assign numerical values to it. Between any two propositions, regarded as premise and conclusion, there exists one and only one such relation. In general, it will not be possible to compare or measure degrees of probability, and therefore it will not always be possible to study combinations of this simple probability relation.

The manner in which Keynes uses or applies his theory is of immediate concern. To Keynes, a/h is the probability of a on evidence or hypothesis h. We know a/h, because we are directly aware of a special kind of logical relation between a and h. By means of this direct intuition we know a follows from h, but not with certainty. Accordingly any properties possessed by degrees of probability are results of any properties possessed by our perceptions of varying degrees of this simple, logical relation between propositions. The principal property which we thus intuitively possess is that given by the Principle of Indifference. In this manner probabilities are assigned numerical values. But it is clear that before the Principle can be applied, judgments of relevance or irrelevance of h must be made. These judgments are fundamental in probability. How are they made?

Keynes, consistent with his rather strong bias against the frequency interpretation, resorts finally to direct knowledge again for these judgments of irrelevance. Evidence is defined as irrelevant, if addition of it to the probability a/h does not alter this probability. For example, in drawing balls from an urn, we must directly judge that color is irrelevant. Similarly, in one of Keynes' examples, suppose black and white balls are drawn from an urn with a magnet, and it is known that the black balls are iron, the white balls tin. Do we then judge directly that the material of which the balls are made is relevant or irrelevant? The answer is that we make such judgments in accordance with our

[6] Keynes, John Maynard, A Treatise on Probability, London, 1921.

knowledge of physics and observed statistical frequencies of the incidence of magnetic attraction in the presence of iron and the absence of such attraction in the presence of tin. From what has been said previously about such judgments of irrelevance, it must be evident that while they may be made directly, they are not plucked from thin air, but are ultimately based on experience of frequencies in conjunction with definite empirical knowledge. As a final estimate, the only valid meaning of a purely logical interpretation of the concept of probability is the establishment of possible events.

It should be noted that Keynes becomes involved in circular reasoning. Probability is based on the Principle of Indifference, hence on indifference. Indifference involves judgments of irrelevance, therefore is based on irrelevance. Irrelevance is defined in terms of evidence which leaves the probability unchanged. This type of circle is not vicious in a purely formal theory, since it is characteristic of implicit definition, but it is necessary to break this circular chain when empirical application is made. Keynes breaks this circle with his interpretation of probability, but the validity of his application is doubtful unless an underlying frequency interpretation is admitted.

THE CALCULUS OF PROBABILITIES AND ITS INTERPRETATIONS

Several times in the previous discussion important criticism has centered on the manner of application of the calculus of probability, regardless of what interpretation of the concept of probability was in question. Consequently, a short explanation of the relation of the calculus of probability to its applications is pertinent.

The relation of the purely formal theory of probability, that is, the calculus, to statistical frequencies as given in reality has that double aspect which every exact science exhibits. On the one hand, there is a purely mathematical structure, developed by logical laws and purely mathematical procedures from axioms or postulates. The calculus is a mathematical system of the type of geometry or mechanics and, therefore, may be regarded in its purely formal aspect as a deductive or postulate system, that is, an extension of the logic of consistency. This structure can only be justified on the basis of the properties of postulate systems and by the correctness of the results to which it leads when applied empirically. The empirical application or interpretation is the second aspect of the duality.

The recent work of Reichenbach, von Mises and others has clarified considerably the nature of this purely formal calculus and its relation

to various interpretations.[7] In general, the systems of Ramsey and Keynes are correctly constructed on the formal side as far as they go (with some exception in the case of Keynes[8]), but difficulties arise either at the point of application or in failure to distinguish the precise point at which the purely formal system relates to material questions.

The aim of a probability calculus is to present a clear, consistent description of mass phenomena or repeated processes. The axioms which lie at the base of this mathematical structure can be given various interpretations or contents, possessing determinate connections with the world of observable data and leading to verifiable, observable consequences. When an interpretation has been given, the theory may be considered in contact with reality from beginning to end, but the system itself is nothing but the purely mathematical structure which stands between the beginning and the end. Thus in its functions, the calculus of probability is analogous to the calculus of propositions. Given elementary probability relations, the function of the system is to furnish derivative combinations of probability relations from the given ones. That is, the existence of probability relations is a material question. But once such relations are introduced into the probability calculus, they can be translated in various combinations within the system.

Reichenbach has recently presented a finely constructed formal theory of probability based on an extension of symbolic logic, but with the addition of one new notion, the probability implication. This theory by permitting the consideration of the form of probability propositions apart from their content has clarified greatly the relation of form to content in probability theory. The probability implication which Reichenbach introduces is constructed in this manner. Consider the proposition 'If a coin is tossed, then the coin comes to rest with H up.' The tosses form a class O; the results form a class P. To each toss x_i of O, there corresponds a unique result y_i of P. The probability implication is a relation between these paired elements, x_i and y_i, and the classes to which they belong, that is, a triadic relation. It is written $x_i \epsilon O \underset{p}{\rightarrow} y_i \epsilon P$. The probability implication differs from the if-then relation of formal logic by substituting for the assertion of truth in the consequent of the proposition 'If a is true, then b is true' the phrase 'probable to degree p.'

[7] Reichenbach, Hans, *Wahrscheinlichkeitslehre*, Leiden, 1935; Mises, Richard von, *Wahrscheinlichkeit, Statistik und Wahrheit*, Wien, 1928; *Wahrscheinlichkeitsrechnung und ihre Anwendung in der Statistik und theoretischen Physik*, Leipzig und Wien, 1931.

[8] Cf. Nisbet, R. H., "The Foundations of Probability," *Mind*, 1926.

Reichenbach uses five axioms: Univocality, Normalization, Addition, Multiplication and a fifth axiom concerning order in the inner structure of probability sequences. To this formal system Reichenbach gives a frequency interpretation and proves his axioms on the basis of this interpretation. Probability is defined as the limit of the frequency in an infinite sequence. There is a geometric interpretation which is also valid for Reichenbach's postulate system, but from the point of view of application only the frequency interpretation is adequate.[9] When anyone uses a probability proposition in science or in everyday life, it is the frequency interpretation which best fits the usage.

This interpretation of the purely formal system is the first step in the application to empirical data. The second step is the actual application of this interpreted theory to objective reality. The latter procedure raises an important problem which cannot be discussed in this paper, the material conditions for the applicability of probability theory. Which interpretation fits reality with the greatest resulting practical fertility and economy is a problem demanding exact investigation and a high order of critical acumen. The interpretation given to the axioms or postulates does not prejudge the applicability to reality.

The division of probability calculus into a purely formal development and a theory with applicational definitions indicates two directions which the historical evolution of the frequency theory has taken. Reichenbach's system represents the development of a purely formal postulate system to which a frequency interpretation is given. An alternative approach, which has been taken by von Mises (also by Dörge, Tornier and Kamke), begins with the frequency interpretation, that is, with the content aspect, and derives therefrom in conjunction with additional requisites all of the laws of the probability calculus. This approach was the first to consider the problem of order within probability sequences, but according to Reichenbach the most comprehensive solution of this problem must come from the formal approach.

To aid in meeting criticisms of the frequency theory in a later section a short exposition of the main ideas of von Mises' development will be helpful. There are three fundamental ideas. First, the theory uses but one concept of probability, the frequency interpretation. In the

[9] This geometric interpretation is in terms of measure as found in mengenlehre. This definition has been developed by Waismann and Wittgenstein. Given a defined class A, the measure mA of this class is a surface segment. Given another class B, having at least one element in common with A, then the probability of the occurrence of an event of B is the ratio of the measure of the conjunction of A and B to the measure of A.

second place, probability is a number measuring relative frequency. A definite characteristic is observed, and the number of its appearances in a mass phenomenon is noted. Third, the probability aggregate or 'collective' in which this characteristic (for example, heads with a coin) is repeated must be precisely defined. If this characteristic appears n_i times, the relative frequency after n repetitions is n_i/n. A collective is defined as a mass phenomenon which satisfies the following two postulates. Postulate 1: When n is indefinitely increased, the sequence n_i/n approaches a definite limit p, called the probability of appearance of the characteristic or element under observation. Postulate 2: If by a 'preassigned' rule a sample is chosen from the sequence n_i/n, then the limit value p remains unchanged. This rule must be independent of the characteristics under consideration, that is, with a coin one chooses every other result, for example, not every other 'head'. Postulate two characterizes randomness and irregularity of mass occurrences. The important point to be emphasized is that a statement of probability has meaning only with reference to a definite collective. The primary given facts are simple collectives and distributions, and the sole object of the calculus of probabilities is to produce derived collectives from the given collectives.[10]

Since the work of Keynes has been widely circulated and greatly influential, it may be permissible to digress at this point and indicate the translatability between his system and that of Reichenbach as regards their formal structure.

It will be convenient to make references to the two theories simply with (R) and (K), and some slight acquaintance with the two theories must be assumed. In (K) the relation of probability P between the proposition a and the premise h is written a/h or aPh. (For simplicity regard h as a proposition.) The correlative formula in (R) is $x_i \epsilon O \underset{p}{\rightarrow} y_i \epsilon P$, where h corresponds to x_i, a to y_i and P to $\underset{p}{\rightarrow}$. The class reference in (R) is emphasized in the frequency interpretation, while it is lacking in (K), or only indirectly suggested by a broader interpretation of h, since h determines a truth-frequency class for a.

If $\bar{a}$ is the contradictory of proposition a, and $\bar{P}$ is the complement of class P, then the Complementary Law in (K) is given as $a/h + \bar{a}/h = 1$; in (R) as $(O \underset{p}{\rightarrow} P) \supset (O \underset{u}{\rightarrow} \bar{P}) \cdot (u = 1 - p)$ or $W(O,P) + W(O,\bar{P}) = 1$, where W( ) is the mathematical form, i.e., the numerical degree of

[10] Mises, Richard von and Pollaczek-Geiringer, H., *Encyclopaedia of the Social Sciences*, New York, 1934, Vol. 12, "Probability."

probability. The Law of Excluded Middle in (K) is $(a + \bar{a})/h = 1$. In (R) this formula is $(O \overset{r}{\rightarrow} P \vee \bar{P}) \cdot (r = 1)$ or $W(O, P \vee \bar{P}) = 1$. The Addition Theorem is in (K): $(a + b)/h = a/h + b/h - ab/h$. The correlative in (R) is best shown in the general addition formula developed later than Axiom III: $W(O, P \vee Q) = W(O,P) + W(O,Q) - W(O,P.Q)$. The Multiplication Theorem in (K) $ab/h = a/bh.b/h = b/ah.a/h$ corresponds to the two formulae in (R): $W(O,P.Q) = W(O,Q).W(O.Q,P)$ and $W(O,P.Q) = W(O,P).W(O.P,Q)$. Independence in (K) is defined as follows: If $a_1/a_2h = a_1/h$ and $a_2/a_1h = a_2/h$, the probabilities a_1/h and a_2/h are independent. In (R), Independence is defined by the corresponding formulae $W(O.Q,P) = W(O,P)$ and $W(O.P,Q) = W(O,Q)$. In (K) by the first formula under independence a_2 is irrelevant to a_1/h. Similarly for (R).

The problem of modality permits an interesting comparison. In (K) certainty and $P = 1$ are defined as equivalent, and impossibility and $P = 0$ are defined as equivalent. Certainty in (K) is defined as the highest degree of rational belief, while impossibility is the relation of minimum probability, that is, if $P = 1$, a follows from h. From Axiom II, 1 in (R) certainty is taken as a special case of $p = 1$, hence from $p = 1$, certainty does not follow. The situation in (R) arises out of the relation between probability implication and material implication. If a material implication holds, then a probability implication of degree 1 holds, but not inversely. This state of affairs was necessary to avoid contradiction. For example, because the possibilities of sticking a needle into any point on a paper are infinite, the probability of sticking a definite point is zero. But some point will be stuck, so impossibility does not follow. To preserve symmetry certainty cannot follow from $p = 1$. Reichenbach states that probability and possibility are disparate concepts whose ranges only partially overlap. Keynes would have difficulty with this problem without extending his definitions of certainty and impossibility to cover the infinite case.

THE CASE FOR THE FREQUENCY THEORY

Every theory which serves reasonable ends corresponds to a definite state of affairs among observable phenomena. For the theory of probability, the most meaningful correspondence to empirically observable facts is obtained through the frequency interpretation. A frequency interpretation provides the only adequate and successful mathematical and empirical theory of statistical phenomena as they are studied in science.

The remainder of the paper will be devoted to summarizing the case for the frequency theory by considering some of the major difficulties in the theory. The exposition will be divided into three parts: the convergence problem or the problem of the limit of a frequency, the problem of the individual case and the objections of Keynes against a frequency theory of probability.

A reference has been made to Ramsey's hearty respect for the frequency theory. Yet he felt that there is a grave logical objection to that theory which precluded his own acceptance of it. He stated the objection in these words:

"There is . . . no established fact of the form 'In n consecutive throws the number of heads lies between $n/2 \pm \epsilon(n)$.' On the contrary we have good reason to believe that any such law would be broken if we took enough instances of it.

"Nor is there any fact established empirically about infinite series of throws; this formulation is only adopted to avoid contradiction by experience; and what no experience can contradict, none can confirm, let alone establish."[11]

This is the convergence problem simply stated. It will be remembered that Reichenbach and von Mises defined probability as the limit of the relative frequency (limit in the mathematical sense). A mathematical series is intensionally given by a law or formula. That is, one can calculate any term and compare it with any other. When an infinite mathematical series has a limit, the limit is determined roughly as follows. First, an arbitrarily small, finite positive number ϵ (epsilon) is chosen. Then it is possible to strike out a finite number of terms N from the series and consider the remaining terms. Now if a limit exists, it is possible to choose ϵ small enough and N large enough such that among all of the remaining terms no two will differ by more than ϵ.

If a relative frequency is to have a convergence to a fixed limit, that is, to a probability, it must be possible to name the large, finite N beyond which all extensions of the sequence $n > N$ give numerical fractions for the relative frequency which remain within the arbitrarily chosen interval, say 2ϵ. For a sequence of observed events or observed relative frequencies such a convergence cannot be admitted for two reasons. First, the probability calculus as a pure mathematical system makes no statements about relations in reality. In the second place, it is contradictory to assume that from a definite N on in the sequence there will

[11] Page 206. op. cit.

be no observed runs of events which will throw the relative frequency out of the small arbitrary finite limits assigned. This is true because such an assumption excludes a whole class of arrangements which possess finite, although very small, probabilities, according to theory. We have the paradoxical situation that we can assume such convergence only if we assume that no embarrassingly long runs occur before a definite place in the sequence, that is, far enough along in the infinite process so that they do not have a sufficiently large disturbing influence. But such an assumption demands too much prior knowledge about the order in which we may expect events. If we lived in a different kind of world, this convergence might be established, but experience being what it is, we are forced for the present to take observed sequences as they come to us.

The definition of probability as the limit of a ratio is necessary in order to develop a consistent calculus on the basis of the frequency interpretation. What can be done about the difficulty? The reply seems to be, for the present, that this definition of probability must be used as a pragmatic rule of operation. The degree of approximation of empirical frequencies to the theoretical convergence must be stated in each context, which means that the number N must be determined anew for each kind of statistical phenomenon. In an unpublished manuscript H. Feigl has advanced the following suggestion: If p_{min} is the smallest probability to be tested statistically in the situation in question, we may assume for practical calculations that $N = C \cdot 1/p_{min}$, where C is a number whose value may be chosen between certain limits namely, $10 < C < 10^3$, the actual limits of C to be set by the experimenter relative to the problem in hand.

There have been other attempts to circumvent this difficulty. A very recent one is that of Lindsay and Margenau.[12] This proposed solution would introduce an 'elementary uncertainty' into the procedure, that is, probabilities too small for experimental detection shall be considered equal to zero. It is not possible to give any further exposition of this problem, except to mention the direction Reichenbach has taken. Because the limit proposition for extensionally given sequences is indeterminate, Reichenbach finds an opposition between formal logic and probability logic with respect to such propositions. Eliminating limit

[12] Lindsay, R. B. and Margenau, Henry, *The Foundations of Physics*. New York, 1936. cf. also Nagel, Ernest, "A Frequency Theory of Probability," *Journal of Philosophy*, 1933; "The Meaning of Probability," *Journal of the American Statistical Association*, 1936.

propositions would eliminate prediction likewise. Therefore, Reichenbach develops a special probability logic with a continuous scale of values, having truth and falsity as limiting extremes. Since induction is used in this process, we may speak of inductive decidability (Entscheidbarkeit) of the limit proposition. But it is not yet beyond controversy that the two-valued logic fails to do all that Reichenbach's continuous-valued logic claims to be able to do.

An ostensibly cogent objection which any defender of the frequency theory must meet concerns the applicability of relative frequencies to individual or unique, non-recurrent events, or stated differently, to singular propositions. For example, if the historicity of an event or individual is in question, the objection is that we cannot examine a statistical sequence of planetary histories to determine the probability of the event in actual history.[13] This is true, but does it do justice to the meaning of probability propositions in such cases?

Consider a proposition concerning the historicity of an event to which the probability degree 2/3 has been assigned by any means whatever. Then we find that either the proposition in question or its negation is compatible with this probability! If a proposition is to have meaning, it must make a difference whether we affirm or deny it.[14] Therefore, the verification of a probability degree seems to be impossible with the individual case. Is there a different meaning?

We judge the trustworthiness of historical sources by assigning a historical statement to a context in terms of the documents which exist in support or rejection of it. Further, historical statements for which documents of a certain type exist can be assigned a certain probability, that is, they can be placed in a class according to certain of their characteristics. Now such a probability has meaning as the frequency of conclusiveness for statements of this type of origin. But this is the frequency interpretation of the concept of probability! That we may lack the necessary statistical information to carry out a strict numerical determination does not alter the fact that a probability statement concerning the historicity of an event has meaning only as a relative frequency of the appearance of certain characteristics.

Allied to this problem is another very difficult problem which has not received a truly satisfactory formulation as yet. What meaning can be assigned to probability propositions about scientific theories which

[13] Cf. Nagel, op. cit.; also Reichenbach, op. cit.

[14] Peirce, Charles Sanders, *Pragmatism and Pragmaticism.* (Vol. 5 of "Collected Papers," ed. by Hartshorne and Weiss), Cambridge, 1934.

themselves contain probability propositions? What context or collective or propositional function admits the corpuscular and wave theories of light as arguments? H. Feigl has suggested that "The probability of a natural law is determined, roughly speaking, by the success-frequency of the inductive method by which it was discovered."[15] This implies that as we ascend in the scale of generalizations, we arrive at some to which we can ascribe no context, therefore, no probability. By pragmatic decree probability does not apply to these generalizations, but only to more special hypotheses. Later scientific advance may supply these higher contexts and raise this arbitrary level. This is a very difficult problem and awaits a more satisfactory solution.

Turning to a consideration of Keynes' arguments against the frequency theory, Nisbet has summarized Keynes' general position somewhat as follows.[16] The frequency theory can be validly applied only to a limited number of probability problems and then only with crippling restrictions. A broader foundation can be given to probability theory, which shows the frequency theory to be a subclass of the broader theory (of Keynes) and also determines the restricting conditions for valid application of the narrower frequency theory. It follows that the broader theory has much wider application than the narrower.

In fairness to Keynes it must be remarked that he is directing his objections against the limited relative frequency theory developed mainly by Venn to that time. Venn emphasized the empirical side of the frequency interpretation, and if we wish to make a liberal interpretation, we may conjecture that the broader viewpoint of Keynes, mentioned in the last paragraph, is the more comprehensive formal development which contains the frequency interpretation as one possible application. Later developments have vitiated the objections of Keynes, but since the work of Keynes is still very influential, it is important to examine his contentions in detail.

The first fundamental criticism concerns the method of selecting the class of reference, the context which gives magnitude to a probability. ". . . as a given proposition belongs to innumerable different classes, how are we to know which class the premisses indicate as appropriate? What substitute has the frequency theory to offer for judgments of relevance and indifference? And without something of this kind, what principle is there for uniquely determining the class, the truth-frequency of which is to measure the probability of the argument?"[17]

[15] Feigl, H., "The Logical Character of Induction," *Philosophy of Science*, 1934.
[16] Nisbet, op. cit.
[17] Page 103, Keynes, op. cit.

In the first place it has been shown that judgments of relevance and indifference are based on observed frequencies and do not have the primary validity that Keynes assigns to them. Von Mises and Reichenbach have amply demonstrated that the frequency theory can give precise rules for the selection of the reference class, while judgments of relevance and indifference are made on the basis of a posteriori data. These considerations and earlier discussion dissolve the objections of Keynes quoted above.

Holding that perhaps this criticism might be met later by the frequency theory, Keynes then proceeds to an attack on the manner in which the fundamental theorems are to be set up by the future frequentist. The objections of Nisbet to Keynes' own theorems will not be repeated here. The arguments which Keynes raises against the addition, multiplication and inverse theorems as based on a frequency interpretation are rather difficult to assess, because they were directed primarily against a position practically unformulated at the time. His objections are basically directed, as before, against the inability of the frequency theory to provide an exact determination of the reference class, a difficulty which no longer exists. Further, by comparing Keynes' objections, as symbolically formulated, with Reichenbach's in the manner illustrated briefly above, one finds additional basic confusions in the criticism. For example, Keynes believes that while in a/h and b/h the hypothesis h may determine the reference class A for a and reference class B for b, there is no reasonable rule in the frequency theory which guarantees that h will also yield any information whatever concerning classes AB (for ab) and A + B (for a + b). Since these conditions are necessary for the validity of the addition theorem, the theorem will not hold in general in a frequency interpretation. A similar argument is raised against the multiplication theorem.

According to the nature of the calculus of probability as presented in this paper, if existence properties of elementary probabilities are introduced into the calculus, these properties are translated into various possible combinations within the defined collective or probability aggregate. Further the reference class must be precisely defined before probability propositions can even be formulated. On the basis of these facts which Keynes did not have access to at the time he wrote, his case does not stand. Further, classes A, B and AB are taken as given and A + B as derivative, a point which Keynes overlooked. But more important is the fact that all questions concerning the reference class are determined before the addition theorem can be applied. However,

it is worth taking the time to refute Keynes' objections in detail, because of their influence.

Along with the problem of the material conditions for the applicability of the calculus of probabilities to the real world it has not been possible to include a consideration of the part which induction plays in probability theory. The main purpose of the paper has been to give a critical and informative survey of the present status of discussion concerning the concept of probability in as simple a manner as possible. The meaning of probability has been the central thread throughout, and the meaning of the concept of probability which possesses universal applicability to the statistical or probability problems of science and a particular congeniality to physical methods is the frequency interpretation. To satisfy the criterion of factual meaningfulness, testability (or confirmability in Carnap's sense), a frequency interpretation of the concept of probability is necessary. This interpretation alone serves with the maximum convenience, simplicity and fruitfulness in the scientific application of probability propositions.

The State University of Iowa.

On Probability and Induction

BY

HANS REICHENBACH

IN A review of my book "Wahrscheinlichkeitslehre",[1] Dr. Ernest Nagel[2] has recently criticized some of my ideas on probability and induction. His review includes a good exposition of my ideas, and I have to thank him for his serious attempts to do justice to my results. He attacks, however, some very essential points of my theory. I may be allowed, therefore, to answer him as frankly and thoroughly as he attacks me.

There are at first some formal objections which I shall discuss briefly. These objections concern the question whether the concept of probability is to be conceived as a semantic term or as a term of "object language".[3] I did not enter into a discussion of this question earlier because it does not possess much relevance for the mathematical and epistemological purposes of my book.

The frequency interpretation opens two ways of interpretation of the probability concept. We may count either *events* or *sentences* concerning events. The result of the first counting is expressed in an object sentence, the result of the second counting in a semantic sentence. According to these two ways of determination we have two ways of

[1] Leiden, 1935, Sijthoff.

[2] Mind XLV, No. 180, p. 501.

[3] I use here the terms introduced by Carnap and Tarski. Object language is the ordinary language, concerning things; syntactic language concerns symbols and relations between symbols; semantic language concerns, jointly, symbols and ordinary objects, i.e. qualities of symbols in relation to facts, such as truth. The transition from a symbol to the designation of a symbol is indicated by quotation marks.

writing a probability term. If we use the predicative conception of probability,[4] these two ways are symbolized as follows:

$$W(\varphi x_i) = p \tag{1}$$

$$W(\text{“}(\varphi x_i)\text{”}) = p \tag{2}$$

(1) is a term of the object language, (2) is a semantic term. (1) is to be read: "the probability of events x_i of the kind φ, within the series of events x_i, is equal to p". (2) is to be read: "the probability of the propositional series '(φx_i)' is equal to p".

It is obvious that both conceptions come to the same so far as practical purposes are concerned, and also in respect to the rules of the probability system constructed. On account of this complete isomorphism the distinction may be dropped. This is what I did in my book. But, although the distinction may be suppressed in the formulas we cannot contest its logical correctness. Strictly speaking, we have to distinguish therefore (1) the *mathematical conception of probability* from (2) the *logical or semantic conception of probability*. In the last chapter of my book, where I pass from the mathematical conception of probability to the logical conception, it is always the semantic conception which I use.[5] The dropping of the quotation marks in the formulas of this last chapter is to be interpreted, therefore, as a convention that the function of the quotation marks is transferred to the parentheses of the symbol W( ). But, I shall never contest the right of adding quotation marks in these formulas, to any one whose logistic inner voice will not submit to this convention.

For the probability implication, I used the form:

$$\text{(i)}\ (\varphi x_i \underset{p}{\rightarrow} \psi y_i) \tag{3}$$

This is to be considered not as a semantic term, but as a term of the object language; it may be conceived, therefore, as a generalization of Russell's formal implication,

$$\text{(i)}\ (\varphi x_i \supset \psi y_i) \tag{4}$$

which is also a term of the object language. Thus, Dr. Nagel's arguments against this analogy are not tenable. I have chosen this form for the probability implication, on account of some epistemological

[4] cf. Wahrscheinlichkeitslehre, p. 375.

[5] This is made clear on p. 376 where I call the probability statement an analogue of tatements of the form "a is true".

considerations: my construction of the probability implication was expressly intended to fit the application of probability concepts in physics, where it is to take the place of the relation of causality. The relation of causal connection between two events A and B is to be considered as the limiting case of a more general relation of probability connection—this was the program from which my construction started. We talk of a causal connection if A is always followed by B; we talk of a probability connection if A is followed by B sometimes only, but yet in a determinate proportion of the cases. This is the intuitive meaning of the statement that the probability implication is a generalization of the formal implication.

In spite of this there remains a difference between both implications. To the general, or formal implication corresponds the individual, or material, implication; there is, however, no such individual complement to the probability implication. I have pointed this out in a previous paper:[6] it originates from the frequency interpretation according to which there is no individual meaning of probability terms.

The relation between the formal implication and the probability implication is expressed by my axiom II, 1:

$$(i)\ (\varphi x_i \supset \psi\, y_i) \supset [(i)\ (\varphi x_i \underset{p}{\rightarrow} \psi\, y_i) \cdot (p = 1)] \qquad (5)$$

According to what I said about the object character of both implications, I cannot acknowledge, therefore, Dr. Nagel's attack against this relation. In my interpretation of (5) I write that certainty is a special case of the probability (1). Dr. Nagel will not admit this interpretation since, according to him, certainty is not a concept of the object-language but, "does involve a reference to a state of belief" (p. 503). I do not think that we should use psychological terms like "belief" in the discussion of logical questions. What we should do instead, is to give an analysis of the different definitions available. Now, there are as far as I see, three definitions of "certainty" in dispute. There is first, "certainty" as an object term. If the event A is always followed by the event B, we say: if A occurs, B is certain. Let us call this the "object-sense" of "certainty". Secondly, "certainty" is used as a semantic term. In the given example, we may say: if the sentence "A occurs" is true, the sentence "B occurs" is certain. This may be called the "semantic sense" of "certainty". As both these meanings correspond

[6] Axiomatik der Wahrscheinlichkeitsrechnung, Math. Zeitschr. 34, 1932, p. 572 cf. also the new paper in Erkenntnis VII, 1937, quoted above.

and differ only in respect to the level of the language, they may be united under the common name of "extensional certainty". Thirdly, we use the word "certainty" to denote the relation of deducibility between sentences. If the sentence '*b*' is deducible from the sentence '*a*', i.e. if the syntactic structure of the sentences shows '*b*' to be a consequence of '*a*', we say "if '*a*' is true, '*b*' is certain". This may be called "syntactic certainty."

Now, it is obvious that I use the extensional sense of "certainty" in my theory of probability. In reference to the probability implication, it is the object-sense I use; within the logical conception of probability it is the semantic sense. (On p. 3F8 I use in this sense the word "necessity" which may be concieved as synonomous to "certainty".) I know there are logicians—and after his remark on "belief", I suppose, Dr. Nagel belongs to those who prefer to see the words "certainty" or "necessity" reserved for relations of the kind of the syntactic implication, i.e. for the deducibility relation. But, I do not see any reason to consider their viewpoint as the only correct one. It is true that in conversational language both significations of the term "certainty" occur; but because of this we cannot say that one of these definitions is the only permissible one.

What we can say, however, is that the difference between extensional and syntactic certainty is not relevant for practical purposes. If a physicist uses the term "certainty" in relation to events, all he wants to say is sufficiently expressed in the extensional concept of certainty. Whether the formula combining his events is tautological, or not, i.e. whether he is to apply also the syntactic concept of certainty, depends on nothing but arbitrary decisions which determine the construction of his formulas. This is to say: if the relation of extensional certainty is empirically verified for two events A and B, we are free to coordinate to A and B descriptions such that there is the relation of syntactic certainty between these descriptions. This is all. *We are free* to construct such descriptions, but we may construct also descriptions of another kind which satisfy only the postulate of extensional certainty. The word "certainty", uttered by the physicist, has as much verifiable content as is expressed in the extensional concept—this, I think, cannot be seriously questioned.[7]

With these remarks, I think, the question for the logical position of

[7] To avoid misunderstandings: it is, of course, verifiable whether there is the relation of syntactic certainty between two formulas. But the question *whether the events demand* the introduction of syntactic certainty *or* that of extensional certainty, is not verifiable.

the probability concept is answered. Probability may be constructed as an object term, or as a semantic term; but there is no need to introduce purely syntactic terms. We shall choose the object conception of probability for mathematical purposes, the semantic conception for for logical purposes. This distinction, however, has only a formal significance as on account of a complete isomorphism both conceptions come to the same result. Practically speaking, what stands at discussion is the question whether we shall count *events*, or *sentences* concerning events—a difference which, as to all formulas occurring, finds its only expression in the introduction of quotation marks?

This is my answer also to a remark in the review of Dr. Louis O. Kattsoff.[8]

II

I have called my logic of probability an extensional system. A remark concerning this term may be added here. The usual definition of this term reads that a relation between propositions is extensional if it depends on the *truth values* of the propositions only, whereas a relation is intensional if it depends on the *intension* of the proposition. Now, the truth tables of the probability logic constructed by me are different from those of two-valued logic: the probability value of a combination of two propositional series is not determined by the probability values of the propositional series, but is dependent on a third parameter which I have called the "degree of coupling" of the propositional series, and which is identical with the probability of one series *relative to* the other. Instead of this parameter, we may introduce also, as a third independent parameter, the probability value of *one* of the logical combinations of the propositional series, e.g. of their logical product. This peculiarity is a necessary feature of the logic of probability as may easily be shown; it corresponds also to the practical use of probability concepts.[9] A. Tarski[10] however, referring to a remark of S. Mazurkiewicz, has based on this fact the objection that my probability logic is *not extensional*. He is right if he uses the usual definition of the term "extensional" quoted above; but I might add then, that my probability logic is *not intensional* either. It seems to me, however, that this is a very narrow use of the word "extensional", and that we should reason-

[8] Philos. Review, Vol. XLV, 6, p. 625.

[9] Cf. Wahrscheinlichkeitslehre p. 386.

[10] Cf. the report on the discussion of the congress of Prague 1934, in Erkenntnis V, 1935, p. 174.

ably expand the definition of "extensional" in such a way that the dependence of the "truth value" of the combination, on further parameters of the "truth value" type, is included. This way of an expansion of a definition, in case of occurrence of problems of a more general kind, is used throughout in mathematics. I refer here to the use of such concepts as "the whole" and "its parts" which, for infinite classes, would lead to contradictions if the older narrower definition of the terms were retained, or to the use of the concept "sum" in vector analysis. If I have used the term "extensional" in my publications, I have of course always referred to such a wider definition. Maybe the objections of Dr. Nagel against my claims of an extensional character for my probability concept originate from a misunderstanding of this usage of the term. My wider definition of the term "extensional" is justified by the request to construct a system of logic which corresponds to the rules of the calculus of probability; keeping to the narrower definition would make the postulate of extensionality exclude those very generalizations of two-valued logic which are applicable to the probability concept. Moreover, all those conditions which make the postulate of extensionality an important epistemological principle, such as the independance of extensional logic of the intension of a proposition, the possibility of basing a theory of meaning on the concept of verifiability, are valid for the wider concept of extensionality as well as for the narrower one.

I turn now to some further objections of Dr. Nagel concerning my probability logic. As these objections are of a more fundamental character, I want to answer them in a more detailed way.

Dr. Nagel asks why I attach so much importance to the introduction of probability logic (p. 509). I have shown, in my book that probability logic may be reduced to two-valued logic by the frequency interpretation; Dr. Nagel wants to interpret this fact as a demonstration that probability logic is dispensable, that it is at best "a more convenient mode of statement". I may add here the remark that some Polish logicians[11] have even drawn the consequence that my probability logic is no genuine multi-valued logic, just because of its reducibility to two-valued logic. Although these objections may seem convincing at first, they turn out to be untenable under a more profound criticism.

[11] A. Tarski in the discussion of the Congress of Prague 1934; cf. the report in Erkenntnis V, 1935, p. 174, and my answer p. 177. J. Hosiasson, Actes du Congrès international de Philosophie scientifique, Paris 1936, IV, p. 58; my answer to this is given in my new paper quoted above, appearing in Erkenntnis 7, 1937.

A multi-valued logic is a relational system defined by truth-tables; according to the way that the truth tables are constructed, different systems of multi-valued logic result. Lukasiewicz and Tarski have developed such systems; another one is my probability logic if it is conceived as defined by my truth tables. Given a set of entities which are to be conceived as the elements of the relational system, the question is to be raised whether these entities fulfill the relations of the system. I have shown that if propositional series are chosen as such entities, and if, we consider as their coordinated truth-values, what physicists call the degree of probability, my system of probability logic is fulfilled. The systems of Lukasiewicz and Tarski may be fulfilled by other entities; but their systems, of course, cannot be interpreted by probability concepts, in as much as the calculus of probability prescribes the rules of probability in a determinate way so that only one relational system is compatible with them.

If we introduce now the frequency interpretation of probability, this may be conceived as the dissolution of the original elements into a new set of elements. We may compare this process to the introduction of new variables into a mathematical function. Let us denote a propositional series by "(a_i)," where the parentheses are to be a part of the notation; we may write then the relational system of probability logic in the form[12]

$$L[(a_i), (b_i)\ldots]$$

Introducing the frequency interpretation means reducing the elements (a_i) to new elements a_i; in relation to these new elements we have then another logical system

$$L_o[a_i, b_i, \ldots]$$

The system L is probability logic; the system L_o is two-valued logic.

It would be entirely mistaken, however, to infer from the possibility of such a reduction that L is to be considered as a not-genuine system of logic. Reductions of the given kind may be invented for every other logical system as well, by the construction of a suitable new set of elements; Post [13] e.g. has shown that his multi-valued logical systems are reducible in a similar way to two-valued logic. The same may be possible for the systems of Lukasiewicz and Tarski as well. We may refer

[12] In this formula, "(a_i)" is not a propositional series, but the designation of a propositional series.

[13] American Journal of Mathematics, XLIII, 1921, p. 182.

also to the example of non-euclidian geometries which, by the introduction of new elements, are reducible to euclidian geometry. Our system L is a genuine multivalued logic in the same sense as the systems of Bolyai and Lobatschefsky are genuine non-euclidean geometries—in spite of their reducibility to the geometry of Euclid.

This remark allows us to give a first answer to Dr. Nagel's objection. If we conceive propositional series as the elements of science, we are bound to probability logic; *for these elements*, probability logic is not a matter of convenience, but a necessary form. A transition to two-valued logic would involve a transition to other elements.

Now there are, indubitably, elements of science which have the form of propositional series. There are a great many scientific statements which do not concern single events, but classes of events; of this kind are statements containing an all-sign, such as physical laws. We know that the truth claims of statements of this kind are not fully justifiable, that we must be content if the predictions they involve turn out to be true, at least, in a great number of cases. We have, therefore, to replace these all-propositions by propositional series to which a determinate probability-value is coordinated. For this generalized conception, an all-proposition would signify the special case when the relative frequency is equal to 1 (not only for the limit, but for every term of the coordinated frequency series); a case which, however, practically does not occur. All these parts of science find their natural logical form in probability logic.

The question arises what we shall do with single-case statements; i.e. statements concerning a single event, at a definite spatio-temporal position. We might conceive them as two-valued entities, however, it turns out that such a determination remains a mere fiction, as we never are able to determine the truth-value in question. All we can do is make an assumption about the truth-value, based on observations; but we must always be ready to change our original assumption, even after the occurrence of the event.

This is due to the fact that every statement involves predictions. If we say e.g. "On February 22nd, at 12 m., Peter was in the Hagia Sofia at Istanbul", this involves the prediction that I shall not get a reliable report from another person that Peter was in the university at that time; that nobody will see Peter in New York on February 24th, etc. If, in spite of that, I should make such a contradictory observation, I should call the first statement false. It is the fact that every statement of physics, including single-case statements, is to be *controllable*, which leads to this consequence; for to be controllable means to involve pre-

dictions. As the class of these predictions is at least practically, infinite, we shall never know the truth value of the statement.

What we have instead is a probability. It may appear strange that I talk here of the probability of a single event, after having so much emphasized the frequency interpretation of probability; I can, however, refer to my book where I have shown that we may, without leaving the frequency interpretation, coordinate a probability to a single case. The single case statement is then to be conceived as a *posit* (I propose this English term for the translation of the German "Setzung"), or as a *wager*; to it belongs a *weight* (I use now this term, in German "Gewicht", instead of my original term "Beurteilung") which is determined by the probability of a propositional series of which the single-case statement in question is an element. As to these concepts, and as to the choice of the corresponding propositional series, I must refer to my book. As an illustration of these concepts, I give the following example: if I want to predict a single throw of a die, the posit "side one will occur" has the weight $\frac{1}{6}$.

We say usually, that *after* the throw has happened, its result is reported by a *true* proposition, and not by a proposition having a probability only. This is however, a schematization; what we have, strictly speaking, is a proposition with a higher probability only or in the terms used above, a posit with a higher weight. This follows from what we said about the possibility of a control, which we must acknowledge for every proposition; the proposition concerning the *observed* throw of the died makes no exception. If we talk, in this case, of a true proposition, we replace a high probability by truth; this is what I call *schematization*.

To understand the logical nature of this schematization, we must enter into a deeper analysis. We spoke of the transition from probability logic to two-valued logic performed by the frequency interpretation; there is, besides, a second transition from probability logic to two-valued logic which we must consider now.

We may divide the scale of probability, reaching from 0 to 1, into three sections, by introducing "partition values" p_1 and p_2 such that p_1 is a bit superior to 0 and p_2 a bit inferior to 1. The probability (or weight) of the statement "a" may be denoted by W(a)[14]; we apply then the following definitions:

$$\text{if } W(a) \geqq p_2, \text{ a is called } \textit{true}$$
$$\text{if } W(a) \leqq p_1, \text{ a is called } \textit{false}$$
$$\text{if } p_1 < W(a) < p_2, \text{ a is called } \textit{indeterminate}$$

[14] According to my remark above I drop the quotation marks.

We call this transition a *transition by trichotomy*. If we omit propositions of the medium domain, as being indeterminate, the rest of the propositions may then be considered as two-valued entities. The choice of the partition values p_1 and p_2 is, of course, a matter of convention.

This is the second transition from probability logic to two-valued logic we spoke of; but it is obvious that this procedure leads only to an approximate validity of two-valued logic. I have explained this elsewhere.[15] The truth-tables of two-valued logic are then not thoroughly valid; e.g. if "*a*" and "*b*" are both true propositions, according to the given definition, their conjunction "*a and b*" may be false. This is because, if $W(a) \geqq p_2$ and $W(b) \geqq p_2$, the arithmetical product of both values (which determines the probability of the conjunction) may be $< p_2$. Only the operation of negation remains correct if p_1 and p_2, as chosen, are symmetrical in respect to 0 and 1.

These are the reasons why the usual conception of science as a system of two-valued propositions is not correct, but valid only in the sense of an approximation. Scientific statements are, either propositional series, or posits having a coordinated weight; their logical system is probability logic.

Jointly with these reflexions, we recognize why the frequency interpretation cannot release us from the use of probability logic. The reduction of the degree of probability to a number determined by the frequency of two-valued propositions, is possible only if we *have* such propositions; what we really have, however, are posits. This reduction, therefore, cannot free the resulting two-valued system from an approximative character either. What is actually performed by the frequency interpretation is the reduction of the determination of the probability of one statement to the determination of the probability of other statements, with the following advantage: the latter probabilities need not be exactly determined, but are classified within the crude scheme of trichotomy only. In this way, the determination of a degree of probability may be reduced to the application of crude appraisals of other probabilities.

This is a typical approximative method. The analysis of its structure shows that the procedure of science cannot be conceived within the frame of two-valued logic, but demands the wider frame of probability logic.

There is a second reason why the frequency interpretation cannot lead us to an exact validity of two-valued logic. It is connected with

[15] Actes du Congrès de Philosophie Scientifique 1935, Paris 1936, IV 26.

the impossibility of knowing the value of the limit of an infinite series, if this series is not intensionally given, as in mathematics, but extensionally given, as physical series are. I have shown in my book, how this problem of the determination of the limit leads into probability logic (§ 68); the statement about the frequency is, as well as the original statements, to be conceived as a posit. Thus, there are two reasons why the transformation of probability logic into two-valued logic by the frequency interpretation cannot be strictly performed: first, the new *elements* introduced are not capable of a strict determination of their truth value, and secondly, the *statement* about the frequency is not capable of a strict determination of its truth-value. The transition from probability logic into two-valued logic by the frequency interpretation is a *theoretical* possibility only; it cannot be *actually* performed, at least not strictly, because the presuppositions of this transition are not fulfilled. Science consists, not of a system of *two-valued propositions*, but of a system of *posits possessing a coordinated weight*: their logic is probability logic. Only approximatively this logical system can be reduced to two-valued logic, by trichotomy and the frequency interpretation; for exact epistemological inquiries, however, the probability character of the logical system cannot be neglected.

A last remark is to be added here. Dr. Nagel reproves me for a "serious mistake" because I "regard 'true' as a special case of 'probable'" (p. 510). He insists that "true" refers to "a relation between a 'proposition' and a 'fact'",[16] and he condemns me because he thinks that my term "probable" cannot be employed in this way. This objection, I think, is settled by my explanation above. Of course, if I call truth a special case of probability, it is the semantic concept of probability I use, not the object-concept; the analogy between truth and probability therefore, can be fully carried through, if I use the predicative conception of probability. Truth is a relation between a proposition and a fact; probability is a relation between a propositional series and a series of facts. If the propositional series is finite, my definitions hold as well; thus if the propositional series reduces to the length $n = 1$, probability specializes to truth—this is what I explain in § 72 of my book. This is the first interpretation of my saying that truth is a special case of probability.

There is, besides, a second one, which is of greater importance. As

[16] I put here the quotation marks beside the terms "proposition" and "fact" because Dr. Nagel does so; I think it however more correct to drop them. We say e.g.: "attraction is a relation between a magnet and a piece of iron"; and not: "attraction is a relation between 'a magnet' and 'a piece of iron' ".

I showed above, what we take as truth is, strictly speaking, a high weight. Thus I may say: what we actually use as truth is a special case of probability. This is not to say that the concept of high weight has exactly the same logical structure as the concept of truth, but that it takes its logical place. A weight is a relation between a proposition and a series of events, and therefore has a structure somewhat different from that of truth. But as we never have truth, we use a high weight as its representative. Thus my statement about scientific truth as a special case of probability may be interpreted: instead of truth, we always use a representative which is a special case of probability.

These are, in short, the reasons why probability logic cannot be dispensed with in science, and why we must consider the truth theory of knowledge as an approximation which, for a more precise analysis, is to be replaced by a probability theory of knowledge.

III

I turn now to the objections made by Dr. Nagel against my application of the concept of probability to scientific theories.

I have developed the idea that the concept "probable" occurring in reference to scientific theories is not different from the concept "probable" in mathematical statistics. I cannot enter here into an exposition of the reasoning which leads me to this conception, but must refer to my book; I will only summarize the main arguments of my theory in the following two statements:

1) The first is that for every proposition containing the term "probable", a coordinated series may be constructed the frequency of which corresponds to the probability of the proposition. If the proposition concerns a single case, the frequency within the coordinated series may be regarded as the probability of the proposition if the latter is conceived as a *posit*, and the probability as its *weight*.
2) The second is that there is no difference, on principle, between an observation proposition and a scientific theory as far as their truth character is concerned; both are claimed to be true in respect to some facts, but can never be strictly verified. There is only a difference of degree.

Dr. Nagel asks me whether my conception is to be "an analysis of the meaning which the phrase 'probability of a theory' does in fact have, or as a proposal to attach a certain meaning to the phrase".

To this my first answer is that every exact definition of a term pre-

viously used in an uncritical way, has the character of a proposal. The author who introduces the exact definition wants to propose that his definition be used in the future—no doubt about that.

This does not exclude, however, the possibility that the definition given can be considered equally well as an analysis of the meaning which the term used up to that time "does in fact have". It is only the wording of the last phrase which I must alter a bit, if I want to show this; for the wording given by Dr. Nagel seems to me to suffer from some indistinctness.

What is the meaning which a term "in fact has"? Is it given by the images or representations which a man actually thinks of when he uses the term? The consequence of such a definition would be that the same term would have different meanings for everybody, because everybody combines different representations with it. If we want to construct an objective meaning of a term we must proceed otherwise. We must ask which meaning we are to ascribe to the term, *if the usual usage of the term is to be justifiable.* I may be allowed to illustrate this by an example. Most people use the term "length of a line" without being able to give an exact definition of it; we should certainly not find a good definition of this term if we should take a kind of average of all opinions uttered by a great number of people concerning the meaning of this term. We may however say: if we interpret "length of a line" as the ratio of this line to another line called "unit of length", we obtain a meaning which, if underlaid to the use of the term in the mouth of most people, makes their statements *justifiable.* This, I think, is the only way to construct an "objective meaning".

If Dr. Nagel agrees to this interpretation of his phrase, I claim for my conception of the probability of theories *objective meaning*, or the meaning which this term "does in fact have". For I can show that if my interpretation is adopted, all our attitude in respect to scientific theories can be justified; the way that a theory is constructed, the fact that we make it a basis of actions, the differences of belief we attach to theories of different probabilities, etc. That is all I want to show.

I do not claim to encompass, with my theory, the opinions of all physicists, or even of a greater part of them. Here I may be allowed to intercalate a remark which, for an empiricist, looks almost like sacrilege; but to which a long experience of intercourse with physicists has led me. If we want to analyse the meaning of fundamental concepts in physics, such as the "probability of a theory", I think, we should not ask physicists what *they mean* by this concept; we should ask them

what *they do* when they use the concepts. As to the question of the meaning of the term "probability of a theory", I confess, the opinion of physicists seem to me, in general, not to be better than those of ordinary people as to the meaning of the term "length of a line". All may be excellent men *in applying* the concept in question; this does not involve, however, that they are able to give a satisfactory *definition* of it. Clarifying concepts demands another mentality than handling them; this is why this task should be left to the philosophers of science. I do not pretend, with this, a kind of superiority on the side of philosophers; on the contrary, I know pretty well that it is just this being focussed on the clarification of concepts which prevents philosophers from doing a good positive work in scientific research. But clarifying concepts is another positive work, and this should be left to the specialists of analysis. This is why I do not approve of the authority of men of science as far as the meaning of concepts such as "probability of a theory" is concerned.

Dr. Nagel writes: "Eminent men of science repeatedly assert that a theory is found satisfactory by them partly on esthetic grounds, partly because they know no alternative theory, and partly because the consequences of the theory have been tested in accordance with a definite technique". (p. 508). This summary of opinions of men of science about their own methods justifies, I think, my sceptical attitude towards epistemological remarks from this side; it shows, I think, that a philosopher should carefully avoid asking a man of science why he believes in his theories. What we obtain by such an inquiry is a kind of religion for personal use, but not a philosophic argument. To analyse Dr. Nagel's quotation, let us take up first the esthetical argument which indeed plays a great rôle in the epistemological writings of certain physicists.

It may be true that a physicist believes in his theory because he thinks it to satisfy esthetic standards; but I do not see any reason why *we* should believe in predictions which are based on esthetic arguments; or why a *technician* should do so. I do not see any relation between esthetic qualities and predictional qualities—and the latter are what a good theory must have. The beauty and harmony of a theory is a matter of taste; it should be easy to construct theories of an extreme beauty which are obviously false. Fortunately, scientific theories are better, mostly, than the epistemology combined with them; the esthetic taste of great physicists coincides in an astonishing way, with the postulates of the principle of induction. I should recommend, therefore,

that no attempts be made to dissuade physicists of this type from their esthetic criterion of scientific truth; they may need it for their personal work—as well as, say, a successful statesman may need the belief that he is sent by God to do his work. But I cannot accept the esthetic argument as anything connected with the validity of scientific theories in an objective sense; i.e. as an argument which makes the acceptance of a theory justifiable.

Surely, a theory with esthetic qualities *may*, later, turn out as true; we cannot deny this. But that is not a sufficient reason to accept the theory. If any one foretells, from the grounds of my coffee, that I am to make a great deal of money in the near future, this may later turn out to be true—in spite of this possibility, I shall not trust this prediction, even though the coffee-grounds theory may have admirable qualities from the esthetic point of view. I do not trust it because there is no inductive relation which leads from the coffee grounds to my making money. There are physicists who are not able to offer better reasons for their predictions than the coffee prophet; but if they are good physicists, there *will be* better reasons—although they themselves do not *know* them. It is the task of the philosopher to show, by analytical methods, the inductive relations which justify a good hypothesis in respect to observed facts. If we should not be able to do this and were to remain with an esthetic theory of the construction of scientific theories, science would not be better than coffee-grounds prophecy.

I may be allowed to tell here a little story which shows that physicists turn out to be much better philosophers when, instead of their own theories, those of a colleague are under discussion. When Schrödinger first published his famous formula of wave mechanics, he presented it within a context full of esthetic and even moral "justifications". He referred to an analogy with the transition from geometrical optics to wave optics; he developed an interpretation of his wave function which was dropped soon after his first publications; he deemed his theory necessary if a "complete surrender of arms" (eine vollständige Waffenstreckung) were to be avoided[17]—in short, he expounded all the arguments which, indeed, had had a great influence on his mind during the process of creation of his theory, but which cannot be regarded as the logical justification of the theory. At that time, Einstein—whom Dr. Nagel quotes against me in a remark concerning his own theories—was

[17] Annalen d. Physik 79, 1926, p. 509.

asked what he thought of Schrödinger's publication. "I skipped the novel" answered Einstein; "the formula is excellent". I do not relate this story with the intention of suggesting to the physicists that they drop the psychological motivation in their publications; these parts of their writings have a high psychological and personal interest, especially if they are written in the charming style of Schrödinger's publications. What I want to say is that arguments of this kind should not be forwarded in a philosophic discussion about the probability of theories. Within the frame of analysis of science, we should always "skip the novels".

If we want to construct a philosophy of science, we have to distinguish carefully between two kinds of context in which scientific theories may be considered. The *context of discovery* is to be separated from the *context of justification*;[18] the former belongs to the *psychology of scientific discovery*, the latter alone is to be the object of the *logic of science*. The confusion of the two kinds of context has become the root of many a misinterpretation of the procedure of science. I confess that the remarks of Peirce concerning the construction of scientific theories, quoted against me by Dr. Nagel, seem to me to suffer from the same confusion. I admire Charles Peirce as one of the few men who saw the relations between induction and probability at an early time; but just his remarks concerning what he calls "abduction" suffer from an unfortunate obscurity which I must ascribe to his confounding the psychology of scientific discovery with the logical situation of theories in relation to observed facts.

I may intercalate here the remark that the distinction propounded is not confined to inductive thinking, but occurs as well for purely deductive operations of thought. If we are confronted by a mathematical problem, say the construction of a triangle from three given parameters, the solution (or the class of solutions) is entirely determined by the given problem. If any solution is presented to us, we may decide unambiguously, and with the use of deductive operations alone, whether or not it is correct. The way in which we find the solution, however, remains to a great extent in the darkness of productive thinking, and may be influenced by esthetic considerations, or a feeling of "geometrical harmony". Nobody would here, in spite of this psychological fact, propound as a philosophical theory that the solution of geometrical problems is determined by esthetic points of view. The *objective* re-

[18] I have used these terms already in a discussion on the Congress of Prague 1934; cf. Erkenntnis V, 1935, p. 172.

lation from the given entities to the solution, and the *subjective* way of finding it, are clearly separated for problems of a deductive character; we must learn to make the same distinction for the problem of the inductive relation from facts to theories.

It is, therefore, the context of justification which we have to consider when we raise the question of the probability of scientific theories. Returning now to Dr. Nagel's quotation, I find that only the third argument presented there belongs to this context, and this argument is of the inductive type. To accept a theory because its consequences have been confirmed by experience is a procedure based on induction; to it corresponds, in the calculus of probability, the so-called rule of Bayes, which permits calculation of the probability of an assumption as a function of its observed implications. Fortunately this argument plays the decisive rôle in the discussion of physicists about their theories; esthetic arguments disappear with a surprizing quickness if inductions based on experiments are against them.

If the inductive relation from the observed consequences to the theory is once admitted, it cannot be denied that there is likewise an inductive relation which supports the theory *before* the consequences are tested. The situation of the theory in respect to facts *before the experimental test* is not different, in principle, from that *after the experimental test*. In both cases there are facts which do not *verify* the theory, but which confer a determinate probability on it; this probability may be small before the test, and great after it. But even before the test there must be facts on which the theory is based; and there must be, also before the test, a net of inductive relations leading from the facts to the theory—else the theory could not be seriously maintained. The net of relations before the test may be of a very complicated kind, whereas the relations from the observational material of the test to the theory may be of a rather simple kind; but in both cases they must be of the inductive type if the adoption of the theory is to be justifiable.

Dr. Nagel states, as the second argument of the given quotation, the idea that theories are sometimes adopted because the physicists do not know any other theory. If this is to be a quotation from opinions uttered by physicists, it would show once more that it is a rather dangerous matter to believe in epistemological remarks of physicists without further interpretation. The physicist who says that he does not know another theory, lets drop the tacit qualification: "of a kind satisfying inductive rules". It is surely easy to construct a lot of theories for any

given set of observations; but most of them are immediately rejected because they do not correspond to inductive rules. The argument in question, therefore, does not apply to the determination of theories in general, but only to the *differential decision*—a problem which we are to deal with later. In any case the argument presupposes the acknowledgement of inductive rules for the establishment of a theory; for without the additional tacit assumption named the argument could not be seriously maintained.

Dr. Nagel may object that he does not talk only of the *opinions* which men of science have about their theories; he wants to maintain besides that the procedure of scientific research is not conformable to a probability theory of hypotheses. He writes: "a careful search of scientific treatises reveals that the probability of theories is not discussed in them, and that in any case no procedures for calculating the probability of theories in the sense specified by Prof. Reichenbach are employed" (p. 508). If this were true, it would be indeed a serious objection against my conception, as this argument is based on the sound intention to derive analysis of science from what physicists *do*, and not from what they *think*. I must answer, however, that just this argument of Dr. Nagel's seems to me to be refuted by every search of scientific treatises which *deserves* the epithet "careful".

For we must be *careful* indeed, in this domain, as the usage of language is frequently built up of concepts which do not openly reveal their logical origin to be in the sphere of probability concepts. The process of schematization which presses knowledge into the frame of two-valued logic, has introduced into this field a great many characteristics which appear remote from a probability theory; we cannot expect, therefore, to find probability considerations other than in a hidden form. If we look, however, with the eyes of the theorist of probability, we shall discover probability relations in all parts of the field of scientific research. We find that hypotheses are to be "simple"; upon a deeper analysis, this concept "simple" turns out to be a typical probability concept, derivable from the inductive principle. We find that hypotheses are not classified as true or false, but are ranked within a scale characterized by such concepts as "likely", "presumably", "surely", etc., thus obviously corresponding to the scale of probability. We find that hypotheses are combined, and that combined hypotheses are considered as supporting each other—a typical form of a probability inference. We find that an increase in the number of confirmations is considered as an increase in the "weight" of the theory—a procedure

justifiable only for a probability conception of knowledge. We find that scientific theories are employed as bases of actions, e.g. for technical purposes; a usage which can be justified if the weight of the theory is interpreted as a probability—whereas all other kinds of interpretation return no answer to this question. I could continue this list to a considerable length if it were necessary.[19]

On account of the schematization of two-valued logic, probability inferences appear frequently in a stunted form. Some probabilities occurring in the full logical structure of the inference may be suppressed because they are very near to 1 or 0; in this way, the form of the probability inference may become unrecognizable. The inference of the detective e.g., from observed traces of a murder to the perpetrator, is a stunted form of the rule of Bayes; so is also the inference from physical observations to the validity of a hypothesis, as we mentioned above. I have used, in my book "Wahrscheinlichkeitslehre", a great many examples of the "qualitative" type which may show the occurrence of probability structures in inferences of every day life, or of non-mathematical sciences; they may serve as illustrations of what I mean by the "hidden" probability structure of inferences which do not openly appear in the form of operations of mathematical statistics.

Whereas the "qualitative" treatment of theories contains the probability structure only in a hidden form, this structure becomes obvious at the moment when questions of a greater exactness are discussed: I mean the application of the theory of errors to scientific theories. It has become usual to omit this subject from the philosophical discussion of theories, as a "secondary matter"; this is one of the serious mistakes in the traditional discussion of the problem. There is no sharp limit between a *qualitative* and a *quantitative* statement of a theory; if we pass, however, to statements of the quantitative type, the schematization of two-valued logic is no longer applicable, and the inferences occurring show their full probability structure. If we say: "the velocity of light is 299796 km/sec, with an average error of ± 4, or of $\pm 0.0015\%$",[20] this means according to well-known rules of mathematical statistics:[21] "the probability that the velocity of light lies between 299792 km/sec and 299800 km/sec, is $\frac{2}{3}$". The hypothesis that the velocity of light is a constant, is a statement of a somewhat

[19] I have given some examples of this kind at another place. cf. Actes du Congrès International de Philosophie scientifique Paris 1935, IV, 6 and 28.

[20] A. A. Michelson, Astrophys. Journ. 65, 1, 1927.

[21] Cf. the author's Wahrscheinlichkeitslehre, p. 226.

more complicated structure. It is confirmed by the measurement on which our given numbers are based, in so far as these measurements were made at different times, and thus (on account of the motion of the earth) in different spatial directions. (The famous so-called Michelson experiment, though based on measurements different from those used for our number, proves the same point but to a much higher degree of exactness.) The general hypothesis differs from the statement concerning the numerical value in that it states less; it may be true even if the numerical value named is false. It can, however, not be freed from the indication of a numerical width of the constant, as an "absolute constant" is not a meaningful physical concept; what can be meaningfully stated is only that the velocity of light is a constant within determinate narrow limits.[22] Thus these limits enter as an arbitrarily eligible factor into the statement. As the transition from the special value of the constant, to the postulate of constancy in general, involves an increase of probability, and as the influence of other arguments in favour of Einstein's assumption signifies an increase as well, we may infer from the statement above: "the probability of the hypothesis that the velocity of light is a constant within an exactness of 0.0015%, is greater than two-thirds". If Dr. Nagel deems a probability as low as two-thirds incredible for a theory as universally agreed to as Einstein's assumption, (I must infer this from an example given by him where even the value nine tenth seems to him to be too low) he may easily enlarge the probability of Einstein's hypothesis by admitting a somewhat greater width of the constant. A simple calculation making use of the qualities of the Gauss function furnishes the result that the probability of Einstein's hypothesis concerning the constancy of the velocity of light is greater than 99.99%, if a width of 0.0052% is admitted for the "constant". This may serve as an example of a case where at least an inferior limit of the probability of the first level may be numerically calculated.[23]

We see that it is erroneous to say that the methods of scientific

[22] This remark needs some qualification. We could eliminate the numerical width of the constant by passing to a statement about a limit occurring for intervals of a width converging towards zero, and might interpret this as a statement about an absolute constant. As the formulation of this statement, however, presupposes other statements admitting a numerical width of the constant, and as statements of this kind suffice for all purposes of physics, we content ourselves here with a statement of this type.

[23] In Erkenntnis 5, 1935, p. 275. I have used for this the term "Wahrscheinlichkeit erster Form". I must refer to this paper for an explanation of the different types of probabilities occurring for theories.

research do not include methods for the calculation of the probability of theories. Whether we actually discover these latter methods within the net-work of scientific proceedings depends only on our ability to see them.

As to probabilities of the second level, we cannot yet determine numerical values. Dr. Nagel objects that we are here placed before a difficulty of principle because we do not know into which class the theory is to be incorporated if we want to determine its probability in the sense of a frequency; if we want to determine e.g. the probability of the second level of the quantum theory, shall we consider the class of scientific theories in general, or only that of physical theories, or only that of physical theories in modern times? I do not think that this is a serious difficulty as the same question occurs for the determination of the probability of single events; I have indicated in my book (p. 391) the way to proceed in such cases. The narrowest class available is the best; but it must be large enough to afford reliable statistics. If the probability of theories (of the second level) is not yet amenable to a quantitative determination, the reason is to be found, I think, in the fact that in this field we have no statistics on a sufficient number of *uniform* cases. In other words: if we use a class of not too small a number of cases, we may easily indicate a sub-class in which the probability is considerably different. We know this from general reflexions, and thus do not try to secure statistics. In the future, more adequate statistics may perhaps overcome these difficulties, as well as the similar difficulties with meteorological statistics have been overcome. As long as we have no such adequate statistics, crude appraisals will be used in their place—as is the practice in all fields of human knowledge which are not yet accessible to better quantitative determinations. Appraisals of this kind, i.e. concerning the probability of the second level of a theory, may acquire practical importance in cases when we judge a theory by the success obtained with other theories in that domain; if e.g. an astronomer propounds a new theory of the evolution of the universe, we hesitate to trust this theory on account of bad experiences with other theories of that kind.

Dr. Nagel adds a remark which he considers as a *logical* difficulty. If we secure statistics on single events, such as the throw of a die, we are to count true or false statements in order to obtain a probability. Such a procedure however—thus runs Dr. Nagel's objection (p. 507)—is not feasible for a statistics of scientific theories, because we do not know whether the individual theory is true or false; the statistics

proposed by me, Dr. Nagel argues, would therefore be logically impossible. This objection reveals one of the erroneous consequences to which a theory of knowledge leads if it does not introduce the concept of probability from the very beginning. Dr. Nagel overlooks the fact that the "truth" of a proposition concerning an event is obtained by a schematization consisting of a transition by trichotomy from a "weight" to "truth". This transition is possible, however, for statements concerning scientific theories as well as for statements concerning a die. All statistics are, therefore, based on counting "posits", and not "true sentences" (I have shown this in §77 of my book); therefore the statistics of theories proposed by me are as feasible as any other statistics. The numerical determination of probabilities by frequencies is not restricted to probabilities of the first level.

I cannot enter here into a more detailed exposition of my conception of the probability of scientific theories. Dr. Nagel asks me to write a more detailed report on my opinions concerning the probability of theories; this desire will be fulfilled as I shall soon publish a new book[24] which will contain, within a comprehensive exposition of the probability theory of knowledge, a closer analysis of this subject. I do not share his opinion, however, that I should have shortened the mathematical parts of my book "Wahrscheinlichkeitslehre". The exact demonstration that the whole of physics can be built up by the inductive rule is contained in the demonstration that the whole calculus of probability (including all applications) can be built up by the inductive rule without any other presuppositions. This is the reason why the mathematical exposition had to be given at such length.

I will briefly summarize my view of the probability of theories. By the application of the rules of probability in a complicated concatenation, observed facts determine coordinated scientific theories with different "weights", or probabilities. The situation *before* an experimental confirmation of a theory is not different, in principle, from the situation *after* such confirmation because in both cases the theory is prescribed by the observations, not unambiguously, but only with a determinate probability; the weight or probability of the theory, however, is increased by a confirmation.

I may add here the discussion of two further questions raised by Dr. Nagel. The first concerns the question of the conventional element in the choice of theories; Dr. Nagel believes that I "overlook the

[24] Experience and Prediction. Chicago University Press, 1937.

large amount of 'convention' (p. 512) which enters into the determination of theories. He then sketches some ideas according to which the deviation from facts, admitted for a theory, is determined by practical purposes.

It is certainly true that this admissible deviation is, logically speaking, a matter of convention; but that does not free us from probability considerations. Whether a deviation observed *will remain* within the boundaries of the deviation admitted, can be determined with probability only; the question, therefore, which is to be answered, from the view point of practical applications of a theory, is in the form: with what probability may we expect that future applications of the theory will furnish results remaining within the practically admissible boundaries of deviation? The convention considered by Dr. Nagel has, therefore, the logical character of an arbitrary parameter without which the question for the probability cannot be formulated; the numerical width to be tolerated for the value of the velocity of light in our example is of this type. The problem of the probability of theories, however, can by no means be eliminated by the consideration of these conventions which concern, not truth, but the formulation of the proposition in question.—The only conventional element in scientific truth is the choice of the partition values p_1 and p_2, occurring in the transition by trichotomy described above; they determine the reliability of truth and may vary according to practical purposes.

The second question raised by Dr. Nagel is whether I believe that *only one* theory is in accordance with observations, as indicated by the inductive rule. The answer is easily to be given if we keep to the principles of the calculus of probability. A set of observations determines not simply *one* theory, but a class of theories, each of them with a specific weight. We may ask whether there is a maximum of the weight; the answer to this, however, cannot be given uniformly. Sometimes there is, and sometimes there is not. There may also be so flat a maximum that we are practically unable to choose between certain theories, whereas others can plainly be crossed out as too improbable. We may call this the case of *differential decision*, analogous to the *differential diagnosis* of physicians (which is, logically considered, a special case of our problem).[25]

It is this case which is usually considered by all those who attack

[25] By differential diagnosis the physicians understand a case where the observed symptoms of illness indicate several diseases as their possible origin, but do not permit a decision among the members of this group unless certain new symptoms can be observed.

the idea of an inductive determination of theories. They emphasize the impossibility of an inductive choice among the small group of theories left to us, and do not see the strong inductive prescriptions which exclude all the other ones. We cannot demand, however, that the rules of probability always determine one choice as the best; it may happen that several possibilities are left with equal chances. It would be entirely erroneous, however, to infer that there are no rules of probability at all valid in this game.

The situation of the differential decision opens the way for decisions based on motives other than of the inductive form; for the application of esthetic motives, or economic motives or what else we want. We must not forget, however, that the narrower decision, in such cases, is not justifiable as determining a *better prediction*, as is a decision based on inductive principles. In the situation of Buridan's ass, we may allow the poor animal to choose the bundle of hay with the esthetically preferable form. If two urns are presented to us, one of which is to contain a lump of gold, we may make our choice by counting the buttons of our jacket—we should not pretend, however, that this procedure involves a method of higher intuition. The probability conception of theories leaves open the possibility of *indifference situations*—no doubt as to that. In such a situation, it is better to choose *one* theory, than *none*; if then we need an external impulse to make up our mind, we may use it.

It is this situation of differential decision in which the result of a subsequent test of the consequences involved by the theory may become decisive. The enlargement of the observational material by the subsequent experiment may be of such a kind that the more comprehensive material leads to the indication of one theory as the most probable one, whereas the original material left equal probabilities for several theories. We recognize here the origin of the conception which makes a difference in principle between the situation before and after the test experiment; which denies any determination of the theory by the original facts on which its first establishment is based, but acknowledges a determination by a confirmation of the consequences involved in the theory. This conception is nothing but a schematization of a special situation which finds its natural place in a probability conception of theories; this schematization is, unfortunately, made in such a way that the inductive determination of theories by facts is dropped. It becomes apparent, however, in the moment we consider not only the

group of theories for choice in the differential decision, but compare this whole group with a wider class of theories.

This analysis may point out the unfortunate consequences arising from a theory of knowledge which presses knowledge into the frame of two-valued logic. A number of essential features of knowledge are dropped by such a procedure, and the way towards an understanding of the method of scientific prediction is barred. This is the reason why I deem a probability theory of knowledge to be the only solution of the problem of the logical construction of science.

Istanbul, Turkey.

Some Theses in the Philosophy of Logic[1]

BY

ERNEST NAGEL

THE following comments represent what seem to me promising lines of approach to some of the broader issues in the philosophy of logic. They are offered, it is perhaps unnecessary to say, with the intent of stimulating rather than foreclosing discussion. And if the conclusions advanced are formulated loosely and with only a crude indication of the arguments which support them, I hope that thereby they will provoke all the more the free flow of discourse which it is the object of this Conference to promote.

Everyone has his prejudices and I have mine; and I wish to begin with a brief outline of those I consider relevant to my theme. It seems to me absurd to suppose that philosophers can achieve a knowledge of the world they inhabit by methods essentially different from those which the ordinary skilled worker in field, factory, or laboratory employs. It is difficult for me to take seriously the various attempts made historically by philosophers to legislate to the sciences just what they can or can not investigate, to set limits to the scope and power of the methods, theories, and other instruments which the sciences use on the basis of a deductive theory of mind and nature, or to "justify" or "ground" the efficacy of their procedures by means of an antecedent metaphysics. Furthermore, it seems to me an integral character of skilled workmanship to insist upon the fact that no statement or proposal has any meaning apart from the methods which are or may be employed to establish or execute them. Consequently, the complex systems of the sciences are not intelligible, nor are questions concerning

[1] Read at the Conference on Methods in Philosophy and the Sciences, held at the New School for Social Research, New York City, on May 22, 1937.

their truth and status legitimate, when these systems are considered outside of the context of the methods of the sciences. This seems to me to be the basis for the frequently made statement that science places greater weight upon the character of its procedure than upon any particular result achieved by its means. And this also suggests why science is a self-corrective enterprise, both as to the details of its methods as well as the conclusions advanced by them, and why a philosophy of fallibilism expresses most adequately the *habits* if not always the verbalizations of scientists. Finally, to conclude my list of prejudices, I believe that the basis for our allegiance to scientific method can only be a *historical* one, namely the untold number of successful resolutions of problems which are to its credit. I am ready to admit as an abstract proposition that perhaps other, non-empirical methods of resolving problems may be discovered and employed; but I cannot take them seriously because of what seems to me the historical fact that the empirical methods of the sciences are alone trustworthy. In brief, the grounds for my preference for the procedures of empirical science are themselves empirical, so that no justification or validation of those procedures is relevant which seeks to base them in some theory of truth, mind, or nature which is established in some non-empirical way.

It is in the light of these prejudices that I venture to make a few brief remarks on the import of recent research in formal logic and probability for the general philosophy of logic.

Logical studies have usually been conducted by philosophers in the context of a definite metaphysics, the latter being employed to support the conclusions reached in the former. In particular, it has been customary to regard formal logic as the most general branch of ontology. The so-called "laws of logic" thus came to be viewed as the most general laws of being, and the forms of propositions as expressing the fundamental relations of substances to inhering attributes. Such a conception of logic is clearly not an account of the function of logical principles in inquiry; it is at best one possible *interpretation* of the results of logical research. But it also seems clear to me that such an interpretation does not contribute anything toward the clarification of the rôle of logical principles, that it is extraneous to the task of the formal logicians, and altogether irrelevant to an empirical study of the function and status of logic.

But in spite of the fact that researches in both pure logic and the conditions of its application can proceed with eminent success *without* such a reading of logical principles, the tendency to ontologize logical

principles has continued even after the original limits of classical formal logic have been broken down through the labors of interested mathematicians. For example, the calculus of relations has been developed amidst the controversy concerning the internality and externality of relations, and the recent discovery (or invention) of a great number of propositional forms and "strange" inferential principles is still taken by many to bear directly on the issues of traditional ontology.

It seems to me, however, that the researches in pure logic within the last fifteen years show a marked tendency to reinstate the ancient but neglected view that the subject matter of formal logic is *discourse*, that propositional forms are *forms* of discourse, and that principles of inference are ways of effecting a *transition* from one piece of discourse to another. We are beginning to see more clearly than ever that it is not at all necessary to go *behind* discourse in order to clarify or "justify" the operation of logical principles, and to recognize that in conducting logical researches we are not exploring the anatomy of Pure Being, but clarifying, refining, and amplifying a specifically human instrument.

Discourse is an instrument for effective coöperative activity. Like any instrument, it may be studied for certain generic features it may possess, and nothing is gained by supposing that the traits of the instrument are simply distorted versions of the traits of the material upon which it works. The traits of discourse which are relevant to the study of the conditions of valid inference are its formal grammar. But discourse is flexible, and while it may be so analyzed that it will seem to express exclusively the alleged relations of substances and attributes, this is only *one* way in which a language may be analyzed—and I think not the most fertile or convenient way. Indeed, the supposition that there is *just one* analysis of discourse—*the* analysis independent of context and purpose—is part and parcel of the well-worn doctrine that *things* have essential natures and that it is the business of science and philosophy to study them. I take it that if there is anything which modern logical research has made manifest, it is that so-called "essential relations" are relations between *terms in discourse*, between elements in an intellectual system, and not between a domain of objects or things each possessing necessary attributes. And I believe, in consequence, that the view that there is such a thing as *the basic logic*, the *real implication relation*, which it is the business of the logician to analyze, is seriously mistaken, and in fact not easily compatible with the discovery (or invention) of the untold multiplicity of logical calculi.

I am therefore suggesting that discourse or language may be viewed

as a complicated calculus, and that the primary subject matter of formal logic is discourse so viewed, and not anything which discourse may represent. Since principles of inference are rules for effecting a transition from one set of statements to another set, the instrumental character of logical principles becomes evident. Indeed, one can go further. If we do regard discourse as a calculus, it seems to me to be a mistake to require a point-to-point correspondence between the various elements of discourse (which also includes the principles of inference) and an alleged realm of antecedent "facts" or "things." For if we take seriously the classical criticism of sensationalistic empiricism,—namely, that "facts" do not come tagged as such, that immediate experience is not primarily or exclusively cognitive, that our statements, whether in every-day life or science, contain a "constructive" element which "goes beyond" what is directly given,—we must also admit, I believe, that our statements are not uniquely determined by or moulded upon such an antecedently completed reality. At least those correspondence theories of truth must be judged as extremely naïve which require a point-to-point correspondence between elements of discourse and elements of experience. Consequently, while discourse may be representative as a *whole*, it need not be with respect to each of its elements. This suggestion is supported by many tendencies in contemporary natural science:—mathematical functions are employed in them which cannot be interpreted in any convenient way as representing either physical objects or relations between them, although the theory *as a whole* does formulate connections between one set of crude statements of every-day experience and another such set.

On the view I am suggesting, therefore, the principles of logic, such as the principle of excluded middle and others, are not to be taken as formulating generic characters of existences outside of inquiry or the traits of all possible being, but as rules by means of which the meanings of our terms is explicated and a transition is effected between various groups of statements. The principles of logic are thus conventional without being arbitrary. They are conventional in that they function *in discourse* and make explicit the various transitions which have been historically developed between various parts of discourse; and they are also conventional in that they help formulate the various *usages* of terms and sentences, and in that they represent implicit *resolutions* to continue a given usage or to conduct deductions in a given manner. They are not arbitrary, however, not in the sense they are fixed by the structure of a reality antecedent to inquiry, but in the sense that they

are shaped and selected by the instrumental character of discourse, by the goals of inquiry and discourse.

There is one further point I wish to suggest. As long as it was supposed that we could have *certain* knowledge of universal laws of nature, —either by means of a direct grasp of "reality" or through the instrumentality of some apriori apparatus—it was obviously pertinent to raise various issues concerning the *truth* of universal propositions which were not simply analytic of the meanings of their terms. With the surrender of these alternative approaches to determining the truth of such statements, philosophic discussion took the turn of evaluating the *probability* of universal or general propositions. But, from the point of view of any attempted analysis of the meaning of "probability" which is committed to clarifying the use of this term in the sciences, I believe that only some form of frequency theory of probability is adequate. Accordingly, attempts have been made to formulate a frequency theory of probability which would be comprehensive enough to include the discussion of the probability of *theories* or universal propositions.

However, for reasons of detail, attempts to formulate clearly a method for evaluating the probability of theories, in this sense of "probability," have run into serious difficulties,—difficulties which arise in large measure from the usual assumption that it is relevant to the business of science to discuss the *truth* of theories. Some recent discussions on probability indicate that we are in fact committing a confusion in supposing that the probability of theories can be significantly evaluated in any sense analogous to the way in which the probability of singular statements can be analyzed. For there is no practicable or clear way in which the *relative frequency of the truth of general propositions* can be determined or even formulated.

We are thus led, I think, by the formal analyses of probability calculi, to the suggestion that we can make a more realistic study of scientific theories, by viewing them not as true or false, but as *instruments* or regulative principles by means of which a connection is effected between one set of singular statements verifiable in direct experience and another such set. The function of a theory is to formulate connections between the statements of every-day experience, and to indicate how we may derive one set of such statements from another set; in brief the function of a theory in inquiry is not to be representative of a hidden inaccessible reality. Theories may be compared on the basis of their fertility in making such connections and effecting appropriate transitions:—that is to say, on the basis of the degree to which they have been *confirmed*;

and we are referring elliptically to this function when we apparently inquire into the truth or even the probability of a theory. This view also has the advantage that it does not require theories to be necessarily interpretable in terms of the qualitative material of every-day experience: for a theory is taken as being a more or less effective instrument for securing transitions from one segment of such a manifold to another.

It seems to me that the history of science supports such a view. If science, taken as a historical enterprise, did in fact aim at obtaining true general propositions, one would naturally expect that scientific theories would converge and approximate more and more closely to some invariant structure of physical nature. In fact, however, we do not find such a convergence. Fashions in theories change, and he would be a bold man indeed who would claim on the basis of the history of science that there is an observable limit to which scientific theories approximate. On the other hand, it seems to me there *is* a definite convergence in statements which are at the level of every-day experience. Such statements as that water quenches fire are practically invariant over centuries of research, and scientific statements about natural constants such as density or specific heats oscillate less and less between convergent intervals as techniques of measurement improve. In this domain of the analysis of the degree of adequacy of theories, some of the most important contributions of applied formal logic are still to be made. I think that if logicians manage to keep their eyes on their subject-matter, namely, the procedures of the sciences, we may confidently expect some valuable clarifications of scientific method which will add to the health both of philosophy and science.

Columbia University.

A Theory of Time

BY

SAMUEL SKULSKY

PROFESSOR SCHILLER'S question, "Must philosophers disagree?" can be answered in the negative if a technique can be worked out whereby it becomes possible to answer the question, "What are they talking about?" It is the aim of the ensuing remarks to provide at least the outline of such a technique and to illustrate its possible effectiveness in the specific context of controversies about the nature of time. That philosophers may continue to disagree in spite of the suggestions offered is, of course, to be expected in view of the long history of failures which compromise positions have encountered. The fate of the conciliator usually has been to receive the contempt of the embattled polemicists, and the fate of his peacemaking, to be construed as just another theory. Of course, this is in most cases the ulterior motive for his pretended altruism in assuming the rôle of arbitrator. Yet there are theories and theories. A position like that of Hume on the question of causality may reasonably be opposed to that of Leibniz but not in the same sense as to that of Kant. For the latter does attempt to provide a bridge between rationalism and empiricism and to do justice to the valid claims of each. The theory of time here proposed immodestly attempts a rapprochement among the multitudinous doctrines of time by setting forth one more doctrine.

Categories have been conceived to be notions indispensable to the elaboration and analysis of a given domain of inquiry, the range of a category being determined by the extent of the inquiry. Thus the notion of space is presupposed in the study of geometry, and mass, distance, and time in physics. The elimination of the idea of number from arithmetic simply annihilates the subject matter, or putting it otherwise, destroys the universe of arithmetic discourse.

Now without prejudging the issue concerning the precise ontological status of categories (their subjectivity, objectivity, neutral status, etc.), it is still possible to examine their relations to the subject matter they define, their relations to each other, and similar issues. For a universe of discourse may with propriety be taken as given and the problems involved in it investigated without attention to theory of knowledge, genetic psychology or any other inquiry not relevant to what I may call the internal analysis of the subject matter. Thus the question whether physical bodies can be analyzed into collections of sense-data is not one of the problems with which the physicist as physicist concerns himself, but a question suggested by interests of another sort from those of physics. At least one of the dominant modern schools of mathematical philosophy, the formalists, have found it necessary to make a similar division of labor in the form of a distinction between mathematics and metamathematics.

Proceeding, then, in the direction indicated above it is possible to discern the major traits which categories in general may be said to possess. First, they pervade their domain so that it is impossible to discover an exception to their proper application. All the entities of arithmetic are thus instances of number, and all the entities of geometry instances of space. Second, and as perhaps a corollary to their pervasiveness, categories may fairly be characterized as necessary to their domains. Most philosophers seem to think these two traits sufficient. But it is surely relevant to ask whether categories have relations to anything other than the objects included in the extension of the universe of discourse. And I am here suggesting that each category has such a relation to the other categories which define a level of analysis, and that this relation is different from that which it has to the entities defined by the universe of discourse. Of this latter relation we may say that particular objects are *characterized* by the category or *exemplify* it, and this is not the relation of categories to each other since no category is an instance of the others defining a domain.

This relation of categories to each other may be called *sustenance* and analogically represented by the relation of the individual stones in an arch. The removal of any one stone from the arch destroys the arch; the imaginative elimination of any one category simply wipes out the subject matter to which the rest may apply and consequently affects their existential status. This implies that where a group of categories define a domain, they do so together or not at all. Hence we may say that they sustain each other; to use a perhaps outmoded terminology, they are related internally and are not independent.

The relevance of these remarks will become clear if we consider the traditional philosophies of time. I think it may fairly be said that most thinkers have employed the method of extended definition. Time is identified with some property or collection of properties and then each is explained, analyzed, and the implications are drawn. There is hardly a notion in the dictionary of philosophical terms which has not served for such identification: space, causality, measure, change, passage, series, truth, difference, activity, order, concreteness, incompleteness, dimensionality, experience, indeterminacy, even meaning. The result has been that most of the writings on the subject have answered the question, What is time? in terms of the procedure characteristic of the special work of the authors dealing with the problem. That is to say, time has served as answer to questions which initially, at least, were not regarded as temporal. In accordance with this procedure, the author defines time in terms of that other category in which his major interest lies and adds the appropriate qualifying, individuating adjectives. Suppose, for the sake of argument, that our major interest is in geometry or, to use a vaguer term, in space. We can then define time in terms of the characteristic notions of this discipline. Instants accordingly become points, durations become lines, irreversibility (time's arrow) becomes a vector property, and the order of instants is defined through the conditions needed to generate a serial order. Now there is nothing wrong in this procedure as an endeavor to reveal the kind of structure discoverable in temporal process, particularly when we remember that geometry has served as an excellent tool in enabling us to exhibit such diverse structures as the solar system and the atom. If time is shown to be isomorphic with certain correlated spatial constructs, it is undeniable that a great deal has been learned about time, perhaps the most important thing for certain purposes. But having achieved this measure of success in spatializing time, the geometric imagination seeks further satisfaction in insisting that the end of the tale has been reached, that nothing further can or need be said. Time is simply the kind of dimension which appears in the relativistic equation for the spatio-temporal interval, the only difference being the addition of the negative sign.

When one asserts that a horse is an animal, one may be presumed to have made a significant and informing statement. What if one were to assert that an animal is a certain kind of horse! Let us imagine a similar situation arising in a dialectical squabble, during which, upon hearing that time is a kind of space, the dissatisfied skeptic announces

that the doctrine fills him with intellectual horror, and that, so far as he is concerned, it makes much greater sense to say that space is a kind of time. Is the skeptic in the position of one who has uttered nonsense? Perhaps. Nevertheless if we accept the testimony of Dr. A. A. Robb (A Theory of Time and Space), we shall have to modify the severity of our ridicule. For it is the skeptic's apparent absurdity which this truly great treatise by a modern Euclid seems to have established as an essentially sober and academically acceptable thesis. Having set forth the definitions, postulates, and theorems accounting for the order properties of time, it is only later that theorems describing space appear. It is true that Robb employs the geometric relations obtaining on two cones having contact at their vertices. Yet the terms denoting these relations are genuinely temporal in their reference: before, after, between, simultaneous, etc.

The moral of the fact that both disputants seem to be right in their philosophies would then be that perhaps time and space have orders or structures which may be correlated. Nevertheless the history of human thought convinces me that most philosophers would not be satisfied with this modest conclusion. They would be only too willing to grant that almost anything can be pictured in a diagram, or at least an algebra of some sort, from the growth of a bacterial colony to the increase in the entropy of the universe. They would, if I read their minds aright, still argue that time is not space just as a colony of living organisms is not the second law of thermodynamics, for time may be correctly correlated with many orders in the realm of existence: with entropy (Eddington), with arithmetic (Kant), with causal sequence (Robb), with variations in truth values (Russell), with motion (Aristotle). Perhaps the Stagirite's identification is the wisest of all since any process may be conceived as a function of time.

It must have occurred to many to ask how it is possible to correlate time with so many diverse domains if it has only one identifiable order of its own. But has it? What is this favored order against which so many others may be deployed? I assume that what is specifically meant by this question concerns the relation of the elements of temporal sequence to each other. This relation is designated by such a term as 'before' or 'after,' and is clearly transitive and asymetric. Other domains with which time has been correlated also involve a transitive asymetric relation. Yet it is possible, at least abstractly, to describe a 'time' that is reversible, and such a two-way process has been seriously considered by G. N. Lewis. It has even been suggested by Pokrowski

that quantum transactions may require atoms of flux, so-called 'chronons,' to account for the transitions from one stationary state to another. In this way violence is assuredly done to the almost unquestioned belief that the temporal stretch between any two successive instants is continuous.

We shall have to draw the inference that it is mistaken to speak of *the* order of time unless we specify the context in which our analysis proceeds. The 'order of time' is an amorphous notion just as 'space' is amorphous unless we specify a frame of reference the perspective of which determines the type of geometry best suited to the description of physical events. Einsteinian relativity has surely taught us that, in a legitimate sense of the term, there may be non-euclidean times as well as non-euclidean spaces.

The mystery usually associated with time derives from the fundamental rôle it plays in all natural transactions. If we seek to designate what this rôle is by specifying what the genus is of which time is a species, we seem compelled to refer to some such even more mystifying term as being: time is a way in which things are. The very language we use to convey our bewilderment is already shot through and through with temporality in the tenses of the verbs with which we are compelled to describe the connections of things. An unavoidable circularity seems to dog our every effort to achieve an understanding of time in terms that do not involve time. But this very frustration may prove to be the clue to the answer we are seeking.

A great deal depends on how we formulate our question. To use the effective contrast which Prof. F. J. E. Woodbridge once employed in a lecture on solidity, we may ask what *time* is or we may ask what time *is*. The first manner of asking the question may be answered with some such appropriate definition as that time is a continuous series of instants. The second manner of asking will insist on the context of discourse in which the answer is to be sought. And this will involve a specification of the categories together with which 'time' makes what we talk about more than hypothetical. It is clear, for example, that we do not get the same answer when we examine the tenses of verbs as when we allow clocks to enter our reflections. Moreover, the 'time' which the psychologist discloses in his 'specious present' may not be the same sort of thing as the 'time' which the physicist measures with his vibrating atoms.

The philosophical imagination has been intrigued by reflections about worlds in which time was not of the stuff of things. McTaggart, Hegel,

and Plato testify to the persistent preoccupation of thinkers with eternal and timeless realms of which this actual world of process and change is at most a distorted reflection or illusory perception by finite human intelligence. Surely the immense amount of energy which these great thinkers have devoted to this kind of thinking is more than willfulness and stupidity. Theirs is a real problem though their manner of stating it is unfortunate and misleading. My guess is that they want to know why the experienced world includes time, what kind of world it is which presupposes time as an ineradicable feature of existence. Perhaps if they had so stated the problem, they would not have had recourse to eternity and ideal essences as explanatory devices. For they would then not have failed to notice not only that experience is temporal but also that it is spatial, causal, changing, metrical, discernible, 'qualified,' ordered and specific; moreover, that there is no favored category. The question then would be whether within the framework of experience defined by observation, experiment, and rational reflection, space does not require time as a conjugate category if it is successfully to serve as the locus of things and events, of actual cubical objects rather than the 'instantaneous cubes' of Mr. H. G. Wells. No matter what grouping of categories is chosen to define a universe, we have on our hands not much more than a system of postulates unless time is supplied as the missing link in the chain of existence. Nothing more mystical is involved in this claim than that if other categories are to have an existential locus, it is necessary to provide notions which have an unmistakeable temporal reference. To define the distance between two points, for example, requires some criterion which insures the *simultaneous* application of the spatial metric to the two points. And what is true of the space of a single observer is a posteriori true of frames of reference whose relations are described by equations of transformation. Minkowski's famous dictum about space and time is more than the utterance of mathematical mysticism of the Hintonian variety.

I may have given the impression that categories are fixed and final, creating by their mysterious interaction, physical, biological, and other types of existence. Such a view of the relations of categories was not at all intended since the only way in which, as the author believes, the categories involve each other is the procedural situation. This implies that the degree of precision required to define temporal relations varies from context to context. The ordinary transactions of daily life may be satisfied by the minute hand on the clock, the race track requires a stop-watch, and subatomic events require statistical techniques. The

result is that the content of our concept of time varies with the other categories with which it is associated and the precision and type of definition they receive. An increase in the precision of definition may even necessitate the introduction of new categories such as the velocity of the signalling medium in relativity physics, or the calculus of probabilities in quantum mechanics.

This conclusion may be reinforced by recalling the just claim of some philosophers that time is an empty abstraction unless taken against a background of change. There are a number of respects in which things can change (position, color, number, size, mass, etc.); there are different types of things undergoing change (feeling states, healing wounds, radioactive molecules); and some changes may be slow and continuous, while others are rapid and discrete. Our concept of time must therefore receive specification in terms of order, measurability, and the types of elements (instants, chronons) of which it is composed. It may turn out that in some contexts, e.g., wave mechanics, temporal stretches are not additive. Newton's dictum about 'absolute, true, and mathematical time' should therefore be regarded as the product of an interest restricted to rational mechanics, and the experiential poverty of an age in which instruments of precision were lacking and the wide possibilities of scientific development were not foreseen.

The concept of time should not be so formulated that it is exclusively an aspect of awareness as such. A category of experience whose ramifications extend to natural transactions which unassisted perception cannot apprehend, must receive a formulation in which extrapolation and complex construction play a large part. No one perceives mathematical instants as such, yet the data of physical science require just such a notion. It is possible to interpret these temporal elements as indefinable primitive notions, durationless dates, after the manner of Euclid's introduction of the concept of point; or as complex notions built up by some such procedure as Whitehead's method of extensive abstraction. But in either case the subject matter no longer is the specious present, or empty time, concepts that may properly enlist the psychologist's interest but are no more significant to the theory of time than perspective vision is to a theory of space, or the gambler's fallacy is to the theory of probability. For though the specious present may be a true description of perception, it is not a condition of occurrences in nature.

The swing of a pendulum is, in fact, a more helpful tool in temporal analysis than all the speculation we could devote to 'concrete, lived

time.' The pendulum, or one of its more exact equivalents, may be used to measure the duration of an act of perception. The converse proposition simply is not true.

The selection of a unit of time in terms of the rhythmic behavior of the pendulum shows us once more that the erection of a concept of time involves us in exploration of a much wider domain, including rigid rods, conservation of mass, the gravitational constant, indeed, the validity of the most basic concepts, principles and laws of physics. What is at stake is nothing less than the possibility of an intelligible view of nature, to achieve which, has thus far, at any rate, required a number of categories selected and mutually adjusted to serve the ends of science.

415 Lefferts Ave.,
Brooklyn, N. Y.

Indeterminisms

BY

RAY H. DOTTERER

RECENT advances in quantum physics have led to a renewal of interest in the problem of human freedom and in the wider problem of indeterminism. It is important to recognize, however, that if every denial of determinism is to be called indeterminism, the latter term covers a wide range of logically distinguishable positions. We should perhaps speak in the plural number of *indeterminisms* rather than of indeterminism. For determinism may be defined, simply, as the doctrine that every event has a cause; or we may add that the effect is always uniquely determined; or that when the effect is complex it can always be exhaustively analyzed into components each of which is the effect of a component of the cause; or that every cause directly or indirectly includes the entire universe. And in the case of each of these, or of still other interpretations of determinism, the denial would be a form of indeterminism.

It is the purpose of this paper to call attention to some of the many determinisms and indeterminisms, and to comment briefly on their adequacy as rival foundations of ethical theory and as proposed descriptions of the relations of events in the real world. We shall see that most of the indeterminisms presuppose a considerable amount of determination; and that some of the determinisms tolerate a good deal of indetermination. Obviously the important question is how much and what kind of both determination and indetermination we should regard as factually real.

The inquiry is complicated by the difficulty of finding a satisfactory definition of the term "cause." In this paper we shall assume that by a *cause* of an event we mean that which the event in question regularly follows. Thus defined the cause may be either a single event or an assemblage of events. To define the cause as that which the event *invariably* follows might commit us unnecessarily at the beginning of

the inquiry. To define the cause as the invariable, or even as the regular *antecedent* of the event in question would lead to difficulty in view of the apparent fact of a plurality of causes in the case of some events. For suppose it to be granted that the same event E may result at one time from A, at another time from B, and so on. How can it be said that any of these is the *invariable* antecedent of E? Even if we suppose the sequence to be invariable, the invariability pertains to the *consequent* rather than to the antecedent. What is asserted is that *if* the cause occurs the effect will also occur. Whenever we have such an "If . . . then . . ." relation between events, we recognize it as a relation of cause and effect.

We have been intentionally vague as to what kind of entities causes and effects are supposed to be. Thus far we have spoken of them as "events." And we may as well continue this usage; for "processes," "states," "things" (unless they are thought of as eternal), or whatever you will, can be regarded as events or complexes of events.

An event is essentially a *change*, an occurrence. But suppose it occurs only once? How then can it be said to follow another event invariably or regularly? It must be admitted that our definition of the cause-effect relationship seems to commit us to the position that a unique occurrence cannot have a cause; for regularity of sequence can be asserted only of events which are repeated. It is important to notice, however, that the term "event," as we have been using it, is ambiguous. Does it mean an individual occurrence, or an occurrence which is thought of as capable of repetition? Let us agree to understand it in the latter sense. An individual occurrence will then be an *instance* of an event. The puzzle of the cause of a unique occurrence may now be solved by introducing the idea of imaginary instances of the same event. To say that a single occurrence has a cause will then mean that *if* a certain antecedent or assemblage of antecedents should be repeated, the event of which the occurrence is an instance would be repeated; that is to say, there would be other instances of it. If there is indeed but a single occurrence of the event, the other instances must forever remain imaginary.

To say that an event is "determined" is, then, equivalent to the assertion that there is an event or assemblage of events so related to the event in question that whenever the former occurs the latter is sure to occur. It may be suggested that we should say, rather, that the latter "tends to occur." For there seem to be cases in which the cause exists and the effect does not follow. In such cases, however, it is

always possible to assume that the "cause" has been taken too narrowly; that if the *total* cause had been considered, if the entire assemblage of events which enter into the production of the event had been included in the "cause," an occurrence of the cause would always be followed by that of the effect. The expression "tends to occur" is, to be sure, a convenient form of words; but only for the reason that the "cause" which is specified is less than the total cause.

Having defined "event" and "cause" and understanding that to be "determined" is to have a cause, we are now ready to ask several questions in the endeavor to distinguish between various meanings of "determinism" and "indeterminism."

1. In the first place we may ask whether all events, no events, or only some events are determined. The theory that all events are determined, in the sense of following regularly upon certain antecedents or assemblages of antecedents, may be called "thorough-going determinism"; while the diametrically opposed position, that no events are determined, would then be "thorough-going indeterminism." The latter theory, however, is not likely to be defended; for it would involve the denial of any regular sequence whatsoever, and such a position would be too contrary to experience to be worthy of serious consideration.

It might indeed be argued with a good deal of plausibility that the same state of affairs is never exactly repeated; and therefore that the whole conception of a regular relation between antecedents and consequents breaks down. But while there is, no doubt, an element of abstraction in our conception of occurrences which are instances of the same event or assemblage of events (as there is in every employment of the conception of *sameness*), it seems clear that we can never get away completely from the experience of routine. Some aspects of our world are capable of repetition; and it seems impossible to deny that *some* events have causes.

The compromise position, that some events are determined and some are not, is accordingly what is usually meant by indeterminism as a philosophical theory. At any rate, when certain philosophers have maintained that ethics requires indeterminism, they do not seem to have meant that there are no cause-effect relationships at all; but rather that, at least within the sphere of human personality, there is an absence of thorough-going determination so that some human choices may be regarded as undetermined events.

If the issue is stated in this way the determinists seem to have the better of the argument. Ethics does not require, and derives no

advantage from the recognition of undetermined events. On the contrary, in their zeal for safeguarding the independence of personality the indeterminists have overshot the mark. For unless an act is the expression of the agent's personality—that is to say, unless it is determined—it is not in any significant sense the agent's act, and he is not responsible for it. Again, if *moral training*, *character*, etc. are to be significant concepts, we must assume that earlier experiences somehow condition later choices and that our knowledge of a man's past gives us some assurance of what he will do in the future. Indeed, ethical concepts are significant *only in so far as* the determinists' account of the interrelations of the various parts or aspects of the world is true.[1] In this regard ethical science is in no wise different from the physical sciences. All alike lose significance in the proportion that events are regarded as beyond the domain of the cause-effect relationship.

2. There is another question which must be asked of both determinists and indeterminists. Whether a given event is regarded as determined or undetermined, must it be supposed to become a factor in the determination of subsequent events? A defender of what we have called, "thorough-going determinism" might conceivably hold that, although every event is an *effect*, it might nevertheless not be true to assert that every event becomes a *cause*. And certainly an indeterminist could interpose no logical objection to the suggestion that events, whether or not they have had causes, may in some cases be without effects. Indeed nobody can prove with absolute cogency that such effectless events do not occur; but it is clear that in the proportion in which they may be supposed to occur, in that same proportion physical science would lose its power to predict. The case is even more serious, if that be possible, for ethical science; since a free choice which would produce no effects would be wholly without significance.

3. We may next inquire concerning a somewhat more important point. If we suppose an event to be determined, in the sense of following regularly from some assemblage of antecedent events, must we assume that the event is uniquely determined, or may there be two or more assemblages any one of which is capable of producing it? We are returning to the question of the "plurality of causes." In order to leave this question open, we were careful to define a cause as that which

[1] Cf. Miller, D. S., *Journal of Philosophy*, Vol. XXXIII, No. 12; Hobart, E. A., *Mind*, Vol. XLIII, pp. 1–27; and McGilvary, E. B., *International Journal of Ethics*, Vol. XLV, pp. 379–398.

the event in question invariably follows, rather than as the regular or invariable antecedent of the event.

It is obvious that in some cases an event which seems to have a plurality of causes—as a headache, a fire, a death—may be shown not to do so in reality. Perhaps the different instances are not really instances of the same event; for example, there may be different kinds of headaches each having its unique cause. Or the seemingly many causes may be found when more carefully examined to have a common factor, which will then be recognized as *the* cause; thus the many causes of fire all include an elevation of the temperature above the kindling point. It is impossible to prove, however, that such a reduction is possible in all cases of apparent plurality of causes;[2] and it may be that this apparent plurality is in some cases a real plurality.

The doctrine of the plurality of causes does not indeed imply indeterminism, in the ordinary sense; but it obviously involves some relaxation of the rigor of strict determinism, as this is sometimes interpreted, in that one could not argue backward with complete confidence from effect to cause. It may, however, be required in order to give a complete description of the physical world. On the other hand, it would not seem to have any significant bearing upon problems of ethical theory; for human choices—assuming them to be determined by factors within the agent's own personality—would be no more and no less free, if they resulted now from this assemblage of antecedents and now from that, than if they were uniquely determined.

4. The converse question may also be raised. Must we assume that an assemblage of antecedents will always be followed by the same consequents? A negative answer would suggest a view which might be called, conveniently but inaccurately, the doctrine of a "plurality of effects." Ordinarily, when what seems to be the same cause is found to produce at one time one effect and at another time another, we assume that the difference is due to a difference in the conditions under which the cause operates in the two cases. Suppose, however, we define the cause as the sum of all the conditions by which the effect is produced,[3] would it still be possible for the same total cause to produce different effects at different times? To be sure, if "effect" and "cause" are treated as correlative terms, and the cause is said to be that which the effect regularly follows, it may be questioned whether "different effects of the

[2] Cf. Mill. J. S., *A System of Logic*, Book III, Chapter X.
[3] Cf. Mill, *Op. cit.*, Book III, Chapter V.

same cause" would be effects at all; but while the terminology is objectionable, the intention is clear enough.

This doctrine of a plurality of effects—or, more accurately, a plurality of consequents—could not, however, be tolerated by science, for it would destroy the possibility of unique prediction. And it would be just as disadvantageous to ethics as to physical science. If taken seriously, it would undermine the concepts of moral training and character in the same way as partial indeterminism; for if the same moral experience can lead to one kind of behavior just as well as to another, no consistent development of personality would be possible. In so far, then, as the doctrine of a plurality of consequents is a true description of reality, in that same degree both ethical science and physical science become impossible.

As long, however, as we limit our attention to the field of macroscopic events such considerations need not trouble us; for in this domain there are no empirical data requiring the recognition of a plurality of consequents.

5. And yet, in the domain of the very small, such data are said to exist. The "principle of indeterminacy" is essentially the doctrine that in this domain unique prediction is impossible. The physicist can predict that the effect of a known cause will be one of a group of events, but he cannot tell which one. The question may still be asked, however, whether the apparent plurality of effects from the same cause is due to the physicist's inability to give an accurate description of the cause, so that what seem to be instances of the same cause may really be different; or whether what is in fact the same antecedent may have any one of several consequents. Let us, for the moment, assume the second of these suggested positions to be the correct one. The assumed situation is then that *if* A occurs, then we are sure that B *or* C *or* D *or* . . . will also occur. We may now ask a further question. Even if we do not know *which* of these will occur, can we say that some of them are *more probable* than others? A negative answer would leave us in the perplexing position in which we found ourselves in the previous section. There would be no sense at all in which unique prediction would be possible. An affirmative answer would indicate that we know a little more than is suggested by the unrelieved theory of a plurality of consequents. Although the result would not be determined uniquely, it would still be determined statistically. And it is this conception of merely statistical determination which has suggested to many minds a renewed interest in indeterminism. The human will, it is said in some

quarters, may have a certain range within which it is free, just as an electron is free within a certain range.

It is hard to see, however, how the kind of indeterminism—if we may call it that—which is suggested by the principle of indeterminacy can have any real bearing upon the problem of human freedom.[4] On any tenable theory of the mind-body relationship the number of intra-atomic events associated with a volition must be very great—so great, indeed, that a merely statistical determination of these must in practice be equivalent to a unique determination of the volition. In other words, the "freedom" of an electron or other intra-atomic unit is wholly irrelevant to the problem of the freedom of the human will.

Indeed the theory that determination is merely statistical can be reconciled with thorough-going determinism, as we have defined it, if we may introduce a change in our notion of that which is determined. In determinism, as ordinarily understood, the effect is in each case an event or assemblage of events. We say, if A then B; or If A (which may stand either for a single event or for an assemblage of events), then B and C and D and E and F, etc. In statistical determinism we have the relation, If A then B *or* C *or* D *or* E *or* F *or* . . . plus a knowledge of the relative probability of some of these alternatives. In the given situation the probabilities are known to be thus and so. It is now the *probability* of an event rather than the event itself which is determined.

6. Let us return to the alternative which was passed by in the previous section. Let us assume that *if* the physicist could arrive at an accurate description of the cause, he would then find—even within the domain of intra-atomic events and certainly in the macroscopic realm—that it would always be followed by a definite effect. The effect may, of course, be complex; but it is, on our present assumption, an assemblage, not merely of possible events, but of actually occurring events.

We may next inquire concerning the relation of a complex cause to such a complex effect. Is the total effect merely the *sum* of the effects produced by the various components of the cause? This question involves the distinction between what Mill calls "composition of causes" and causation in accordance with "heteropathic laws."[5] The latter kind of cause-effect relationship is now usually spoken of as "creative synthesis" or as "emergence." And C. Lloyd Morgan's distinction between "resultant effects" and "emergent effects" appears to be the same as that made by Mill.

[4] Cf. McGilvary, *Op. cit.*; also Drake, D., *Invitation to Philosophy*, pp. 403ff.

[5] *Op. cit.*, Book III, Chapter VI.

Morgan illustrates the distinction by pointing to the properties of chemical compounds. On the one hand, we have the molecular weight, which is merely the sum of the weights of the atoms which constitute the molecule; on the other, certain properties of the compound which are different from those of the elements entering into it. Contemporary theories of atomic structure may weaken the argument somewhat; for it may be that if we only knew enough the properties of the compound could be shown to result necessarily from those of the elements which are compounded. And it may be, in general, that the apparent impossibility of analyzing the effect exhaustively into component effects each of which can be shown to follow from the components of the cause, is a consequence of our imperfect knowledge of both cause and effect. Nevertheless it is impossible to prove that this is true universally; and the conception of emergence seems to be unavoidable,—especially when we consider biological problems, such as the beginning of life or of each individual stream of consciousness.

This account of the cause-effect relationship may also be regarded as a kind of indeterminism; for in so far as the complex effect contains elements not accounted for by the components of the cause, the theory certainly involves some relaxation of rigorous determinism. Readers of Nicolai Hartmann's *Ethics* will recall that he bases the ontological possibility of what he calls "positive freedom" upon the recognition of the emergence of a "categorial novum" in cases where an effect results from a complex set of conditions. He maintains that "there is no bondage of the categorial *novum* as such to the elements which it catches up into itself and upon which it mounts; . . . Above them it is, with all that proceeds from it, categorially 'free,' autonomous. It has its specific determinant in itself."[6]

7. Hartmann's account of the manner in which creative synthesis is supposed to safeguard personal responsibility is, however, extremely vague. In the endeavor to clarify the problem a further question may be raised. Even if the effect is not exhaustively analyzable into components resulting respectively from the components of the complex cause, may we still assume that the same total cause always gives rise to the same total effect? If not, if the emergent "effect" may be different without any relevant difference in the complex of conditions by which it is occasioned, then we encounter the same difficulty as in partial indeterminism and in the doctrine of a plurality of consequents (of which

[6] Vol. III, p. 242, translated by Stanton Coit.

this would indeed be a special case). Both ethical science and physical science are possible only so far as this theory is not true!

In this connection it may be well to call attention to the words of Hartmann himself: "The intervention of any entity which pursues ends in the world in which it exists is only possible in a world causally determined. . . . In a world without law and determinism, . . . an agent who pursued ends could not hold his own at all."[7]

On the other hand, if the emergent effect is assumed to be regular and therefore predictable, the doctrine of creative synthesis would be practically equivalent to thorough-going determinism; for the complex effect would always have a cause from which it regularly follows. It seems clear, then, that this doctrine, while apparently unavoidable in giving an account of processes taking place in the physical world, is of no advantage in ethics. At worst, it destroys the conception of moral responsibility; at best, it is morally irrelevant.

A little later, however, we shall see that ethics does require the kind of indeterminism presupposed by *pluralism*, which while not necessarily denying that every event is causally determined, yet insists upon the partial independence of different streams of causal influence. In so far, then, as pluralism insists upon some lack of necessary connectedness among the events of the real world, the doctrine of creative synthesis serves as a psychological preparation for it.

8. Our definition of a cause as that from which an event regularly follows suggests, in addition to the points already considered, some questions concerning the spatial and temporal aspects of the cause-effect relationship. One of the more obvious of these is the question of "action at a distance." If the cause of an event is simply an event or assemblage of events from which it regularly follows, then any event at all which regularly occurs just before it would seem to qualify for recognition as its cause or as part of its cause, regardless of the amount of intervening space.

There are, however, two considerations to which attention should be called. In the first place, we encounter the practical or operational difficulty of verifying regularity of sequence in the case of events which are at a great distance from each other. In the second place, the recognition of the direct causal efficacy of events which are remote from us would vastly complicate experimental procedure. On methodological grounds it seems necessary to postulate that far-off events, except as

[7] *Op. cit.*, Vol. I, p. 277.

they operate indirectly, may be safely neglected when we seek for the causes of those that are near-by. We should make little or no headway in the explanation of terrestrial phenomena if we had to take into account the possibility that changes in distant stars might participate directly in their production.

Reasons such as these no doubt explain our contemporary preference for "field theories" as against theories of action at a distance. From our present point of view the denial of action at a distance may be thought of as a very modest kind of indeterminism. Instead of entering into direct causal relation with events in our neighborhood, the far-off event produces events in its own neighborhood, which in turn produce events in their neighborhood, and these in theirs, and so on, until by this indirect process the events in our neighborhood are finally produced. By a not too violent figure of speech, this train of intermediate effects each of which in turn becomes a cause, may be regarded as an "influence" which requires time to travel from one region of space to another.

9. Thus the relation of space to causation merges with that of time. We must now ask, Is it possible for a remote past event to enter directly into the causation of a present event? There would seem to be no entirely cogent reason for either an affirmative or a negative answer to this question. The same considerations, however, which led us to give up action at a distance in favor of action between spatially contiguous events would lead to the corresponding doctrine in the case of time. It seems to be more convenient in this case too to distinguish between direct and indirect causation and to limit the former to events which come immediately before the event in question.

A person may wonder, indeed, just how much time may elapse between two events one of which can nevertheless be said "to come immediately before the other"; or how far apart "spatially contiguous" events can be! And neither of these perplexities can be resolved; for events which are "immediately" successive are not quite contemporaneous, and events which are "contiguous" are not exactly in the same place. There seems to be no reason, then, why we should absolutely deny direct causal agency across a large spatial or temporal interval, while assuming it to operate across a very small interval.

A compromise position might be suggested to the effect that neither spatial nor temporal remoteness constitutes a bar to direct causal agency; but that this falls off so rapidly with increasing separation that in practice only what we have called indirect causal influence need be taken into account. This hypothesis could not be disproved; but, on

the other hand, no reason could be given for the assumed falling off of direct causal agency with increasing separation. Such an assumption seems natural enough in the case of an influence which travels, and may therefore be supposed to spread over a larger and ever larger cross-section; but it is not at all obvious that it should hold for a causal agency that is supposed to operate directly.

Returning to the theory of indirect causation of events that are spatially or temporally remote from their causes, there is, as already remarked, an intimate relation between the two kinds of remoteness. If time is required for an influence to travel, as is assumed by field theories of causation, a spatially distant event must be sufficiently remote in time or it cannot exert any influence. It would follow that contemporary events, unless spatially contiguous, can have no influence upon each other.

The denial of direct causal connection between temporally remote events may also, if we choose, be regarded as a kind of indeterminism. It is doubtful, however, whether the hypothesis of the causal independence of events which are spatially or temporally separate has any bearing upon ethical problems, except as the recognition of a lack of relatedness between some of the events which make up our world may tend to make our minds more hospitable to the position suggested in the next section.

10. The last two sections have their chief significance as leading up to the question which must now be faced. Does the total cause of every determined event include, directly or indirectly, the entire past of the universe; or are there some events which are caused by only a limited part of the past of the universe? If it is assumed that every event is determined, both of these alternative positions are kinds of determinism. The former may be called "singularistic determinism"; the latter, "pluralistic determinism." In so far, however, as pluralistic determinism attempts to set limits to the interrelatedness of the events which constitute our world, it may be regarded as a kind of indeterminism. It seems, indeed, to be the only kind of indeterminism which is required by ethical theory.

The singularistic theory would merge all so-called finite persons with the all-embracing Whole. For, if every event enters directly or indirectly into cause-effect relationships with every other event, past or future, far or near, then "my" act is an act of the universe as a whole, and in no significant sense *my* act. In short, on the singularistic hypothesis, it is impossible to find *moral subjects*.

We find, then, that ethical theory requires both determinism and indeterminism. It requires determinism in order that the moral subject may be rightly regarded as the author of what are called its acts, and that these acts may have consequences; it also requires indeterminism in order that the moral subject shall not be swallowed up in the Whole.

Both of these requirements are fulfilled by the pluralistic form of determinism. It asserts that every event has a cause, but does not regard the cause as including the whole universe. Instead it departmentalizes its universe, postulating the existence of a multitude of relatively independent strands or streams of causal influence and thus permitting us to believe in the freedom of finite selves.

Pluralism may, of course, be combined with partial indeterminism. As a matter of fact, there may be some uncaused events. We may go further and grant that it is more probable than not that uncaused events now and then occur. Those who, like William James,[8] have a sporting interest in novelty may indeed rejoice in the thought of events, themselves uncaused, which enter into the causation of subsequent events. Our world is pretty bad; perhaps a random reshuffling of the cards will give us a better hand. Thus indeterminism becomes a gospel of hope—of a gambler's hope in something radically new and different. Those who are less willing to take chances may, however, point out that such random reshuffling is just as likely as not to produce catastrophe. Dependence upon chance for the realization of ideals is not the way of intelligence; for intelligence demands order and uniformity of sequence. Indeed, as we saw in the first section of this paper, the occurrence of very many uncaused events, far from being humanly desirable, would destroy the possibility of both science and moral endeavor. In short, we concede the probability of uncaused events, but do not wish for their occurrence. Pluralism has moral significance only in so far as it is deterministic as well as pluralistic.

Determinism must be pluralistic in order to preserve the independence of moral subjects. It need not, however, become a doctrine of "windowless monads." In particular, in accounting for the behavior of human persons we cannot refuse to recognize the rôle of environmental stimuli. Persons, as we know them, are not altogether free from external determination. Yet the world *stimuli* itself suggests that the principal source of determination is within the organism; for not all environmental processes become stimuli. The organism, by virtue of its own con-

[8] See *Pragmatism*, pp. 118ff.

stitution, *selects* those influences to which it will respond. Different organisms respond differently in the same environment. Pluralistic determinism generalizes this well-established biological principle. It recognizes that in seeking to account for certain events or processes certain other events and processes may rightly be treated as irrelevant.

We conclude, then, that every theory of the structure of reality except singularistic determinism includes an element of indeterminism; for each of them insists upon some lack of connectedness among events. Partial indeterminism—the denial that every event has a cause—may be true; yet if the number of undetermined events were very great both physical science and ethical science would be non-existent. The "indeterminacy" admitted by recent physics does not seem to have any important bearing upon the problems of ethics. "Free will" must be admitted to involve determination and to be inconceivable without it.[9] On the other hand, the conception of moral responsibility presupposes the kind of indeterminism that is involved in a pluralistic view of the world.

The Pennsylvania State College.

[9] Cf. Hobart, E. A., *Mind*, Vol. XLIII, p. 1.

Émile Meyerson on Scientific Explanation

BY

OWEN N. HILLMAN

IN THE works which constitute his distinguished contribution to philosophy,[1] Emile Meyerson has advanced and defended the opinion that scientific explanation consists in transforming empirically discovered natural laws into statements of identity in time.[2] This contention, which it is the purpose of the present paper to examine, is of great interest both on its own account and by reason of its intimate connection with Meyerson's central thesis that all thought consists essentially in similar processes of identification. Indeed so intimate is this connection that our present inquiry although it deals explicitly only with the nature of scientific explanation nevertheless constitutes by implication an examination of the more general thesis also, for if scientific explanation is not a process of identification then obviously not all thought is identificatory.

Science has, according to Meyerson, two correlative activities:[3] the one descriptive, the other explanatory. The first of these activities he calls "legal" science, while to the second he gives the name explanatory or causal science. "Legal" science is concerned with the discovery of

[1] Identité et Réalité, 4th ed., Felix Alcan, Paris 1932.
De l'Explication dans les Sciences, 2ed imp., Payot, Paris 1927.
La Déduction Relativiste, Payot, Paris 1925.
Du Cheminement de la Pensée, Felix Alcan, Paris 1931.
These works are hereafter referred to by the letters I.R., E.S., D.R., and C.P.

[2] "En recherchant l'explication d'un phénomène, ce que le physicien poursuit en réalité, c'est la démonstation que l'etat conséquent ne diffère point du présédent, mais peut au contraire être considéré comme lui étant identique". C.P. p. 49.

[3] "mais la science n'est pas qu'explicative, elle est encore *légal.*" E.S. p. 669. See also I.R. p. xviii, E.S. p. 48, E.S. p. 60, and I.R. p. 438.

the laws according to which natural events take place, while causal or explanatory science has for its function the explanation of the laws which "legal" science discovers.[4] "Legal" science, he affirms, owes its origin to our need, and consequent desire, to predict the behavior of our environment.[5] But explanatory science, he assures us, owes nothing to this source.[6] It has no practical ground for it springs solely from the desire of the human mind to understand nature.[7] The human mind is not satisfied merely with knowing *how* natural events take place[8]—which is all that "legal" science tell us—it also wishes to know *why* natural events take place as they do. Now when we ask why an event takes place as it does what we are asking for is something a knowledge of which would enable us to see that the event could not have failed to be, nor have failed to be exactly what it is. We are in short asking for something that rationally necessitates the event.[9] So long as an event could, for all we can see, have failed to occur, or have been different from what it is, so long we do not understand it; but just as soon as we find something that shows us that the event was necessary we understand it and our curiosity concerning it is satisfied.[10] Thus to explain an event is to exhibit its necessity.[11]

The laws of nature are the laws according to which events succeed one another in time, for since we are able to observe only time, i.e. successively, it follows that empirically discovered laws must in reality be statements concerning the succession of events in time.[12] What therefore, explanatory science has to do is to exhibit the necessity of the laws according to which natural events succeed each other in time, that

[4] For Meyerson's arguments that science is both causal and "legal" see I.R. Chap. I and E.S. Chaps. I and II.

[5] I.R. p. 9.

[6] I.R. p. 35.

[7] I.R. p. 36 and E.S. pps. 45–48.

[8] E.S. p. 48.

[9] "Ainsi la raison d'un phénomène doit avoir une nature telle qu'elle suffise à déterminer ce phénomène dans notre raison." E.S. p. 66.

[10] "Ainsi ce que nous cherchons, quand nous parlons de comprendre un phénomène, c'est à le concevoir comme nécessaire, à "faire voir qu'il dépend nécessairement de jugements nécessaires", comme le dit excellemment M. Lalande à propos précisément du terme *expliquer*" E.S. p. 68.

[11] E.S. pps. 65–68, E.S. p. 182, and I.R. pps. 17–18.

[12] "Comme nous ne pouvons observer que dans le temps, successivement, la loi empirique revient en réalité à une loi de succession des phénomènes." I.R. p. 2.

is to show that the invariant sequences which "legal" science discovers empirically are necessary sequences.[13]

But how can laws of temporal succession be transformed from contingent facts into necessary connections? This transformation can be effected, Meyerson affirms, by the process of identification, that is by establishing a fundamental identity between the antecedent and its consequent.[14] Causal science explains why one event succeeds another by pointing out that the two events are basically identical and differ only in respects which are from the scientific point of view insignificant and therefore negligible. By showing that the antecedent is identical with the consequent we prove the antecedent to be a sufficient reason for the consequent,[15] and thus transform a merely empirical succession into a necessary sequence.

But at this point we are confronted with a difficulty. Is it really true that, as Meyerson asserts, a thing's having existed in the past is a sufficient reason for its continuing to exist in the present? Clearly it is not. For if the existence of a thing in the past is a sufficient reason, i.e. necessitates, its present existence, then obviously nothing that had once existed would ever pass away. If what has once existed can subsequently cease to exist, then it is not true that whatever existed in the past must necessarily exist in the present, and if it is not necessarily true that what existed in the past must continue to exist in the present, then the past existence of a thing does not necessitate, and hence is not the sufficient reason for, its present existence. But that things do pass away can scarcely be denied, and since they do, past existence is no guarantee of present existence.

Statements of identity in time are in reality no less contingent than laws of change. For, since some things perish some statements of

[13] "Ainsi ce que nous cherchons, quand nous parlons de comprendre un phénomène, c'est le concevoir comme nécessaire." E.S. p. 68.

[14] "Nous, avons vu, en effet, que la physique entend rattacher l'antécédent et le conséquent par un lien rationnel, en démontrant que le conséquent est le conséquence *nécessaire* de l'antécédent. Or, Leibniz nous l'a dit, les vérités nécessaires doivent pouvoir se réduire à des vérités identiques. C'est donc que nous devrons pouvoir démontrer que le conséquent est identique à l'antécédent—sauf bien entendu certaines particularités qui l'en distingueront, mais que déclarerons . . . négligeables. En d'autres termes, il doit y avoir, si un phénomène doit paraître expliqué, rationnel, égalité entre la cause et l'effet." E.S. p. 157.

[15] "Qulle, peut être la raison déterminante de l' être conditionné par le temps? Il n'y en a qu'une seule possible: c'est la préexistence. Les choses sont ainsi parce qu'elle étaient déjà ainsi antérieurement." I.R. p. 38.

identity in time are false; and since some statements of identity in time are false, no statement of identity in time is, merely in virtue of being such a statement, necessary. But if statements of identity in time are not as such necessary how can the process of identification have any explanatory force? Of what use is it to transform laws of change into statements of identity in time if the latter are quite as contingent as the former? In order to understand Meyerson's answer to this question it will be necessary for us to reconsider the nature of explanatory science from a slightly different point of view.

Explanatory science has, according to Meyerson, as we have already seen, no practical ground. Its aim is solely to establish as complete and perfect an agreement and correspondence as possible between the order and arrangement of our thoughts and the order and arrangement of things.[16] Explanatory science must conform on the one hand to nature as we experience it, and on the other to the mind's demand for rationality.[17] In order to satisfy the mind's demand for rationality explanatory science must establish that the natural laws which "legal" science discovers are necessary laws for a law is rational (i.e. we understand it) when and only when it is evident to reason that the law could not have been other than it is.[18] Now there are only two ways in which the necessity of a natural law can be exhibited, namely either (a) by showing that the law in question is necessary *per se* or (b) by showing that it is rigorously deducible from laws which are necessary *per se*. Thus in order to establish the necessity of a natural law either that law, or some other from which it can be inferred, must be shown to be necessary *per se*. There is, however, only one proposition that is necessary *per se*, and this is the tautology *A is A*. For the sole absolute necessity that reason acknowledges is that of simple self-identity—i.e. of a thing's being what it is.[19] Explanatory science is thus confronted with this

[16] "Ainsi tout raisonnement—du moins en tant qu'il essaie de pénétrer dans la nature—constitue nécessairement une rationalisation de celle-ci, en ce sens qu'il tend à montrer que, par un côté, sa marche est conforme à celle de notre raison." C.P. p. 52.

[17] "La science, obéissant à la tendance explicative qui est le propre de l'esprit humain, a bien l'air de vouloir tout réduire au rationnel. Mais, d'autre part, infiniment soucieuse de garder le contact avec la nature, elle reconnaît les limites que celle-ci impose à ses efforts." E.S. p. 187.

[18] "Le but que vise toute explication, c'est, dans le sens le plus général, de nous faire comprendre le monde, que nous n'apercevons tout d'abord que comme une vérité de fait, vérité accidentelle par conséquent, comme quelque chose de nécessaire, comme une vérité de raison." E.S. p. 182. See also E.S. p. 68.

[19] E.S. p. 137.

dilemma. In order to reach its term it must exhibit the absolute necessity of the laws which "legal" science discovers, and yet it cannot do so inasmuch as the only absolutely necessary proposition is the altogether empty tautology *A is A* from which nothing whatever can be deduced.[20]

But although explanatory science cannot *reach* its term it can none the less make progress *towards* it. The laws of nature cannot be perfectly rational,[21] that is bare tautologies, for we cannot without destroying the very laws we seek to explain deny the temporal disparity of antecedent and consequent, but we can, by setting aside as insignificant the qualitative differences between the two, show that the temporal difference makes no difference and thereby render the temporal disparity itself insignificant and negligible. In so far as antecedent and consequent are identical so far it is precisely as though no time had elapsed.[22] Hence the more completely we identify antecedent and consequent the nearer we come to eliminating the passage of time itself, and the nearer we come to eliminating the passage of time the nearer we come to simple self-identity and consequently to perfect rationality.

It thus turns out that the mind's search for rationality is in fact the search for identity in time[23] since statements of identity in time are of all propositions not purely analytic the most nearly rational.

Further, since the mind by its very nature desires rationality,[24] the nearer a proposition comes to being a statement of simple self-identity, that is the nearer it comes to satisfying the mind's demand for rationality, the greater is our predisposition in its favor. Indeed so great is this predisposition that any and every statement of identity finds our minds extraordinarily willing to accept it.[25] All statements of identity are, therefore, *plausible*,[26] that is to say they command an

[20] "Mais, de ce principe, (i.e. the principle of identity) nous ne pouvons tirer par déduction aucune proposition précise: . . . I.R. p. 463.

[21] "Aucun phénomène, même le plus insignificant, n'est complètement explicable." I.R. p. 459.

[22] E.S. pps. 157–159.

[23] "En recherchant l'explication d'un phénomène, ce que le physicien poursuit en réalité, c'est la démonstration que l'etat conséquent ne diffère point du précédent, mais peut au contraire être considéré comme lui étant identique." C.P. p. 49.

[24] E.S. pps. 45–46.

[25] "Toute proposition causal, explicative, c'est-à-dire visant la réduction à l'identité, trouve notre entendement merveilleusement prêt à l'accueillir . . . . " I.R. p. 464.

[26] For Meyerson's account of the *plausible* see:
I.R. pps. 161–162 and p. 464.
E.S. p. 568.
C.P. pps. 618 et seq.

assent that does not depend upon the frequency and regularity of their empirical confirmation for, owing to our predisposition in favor of identity, we have a conviction of their truth that is at once prior to, and independent of, experience.

Plausible propositions, that is statements of identity, are neither *a priori* nor *a posteriori*. They are not *a posteriori* since they elicit a conviction of their truth for which their empirical evidence alone does not account, and they are not *a priori* since they are not characterized by apodeictic certainty. They stand, therefore, intermediate between the *a priori* and the *a posteriori* different from either yet having something of the character of each.[27]

But although all statements of identity are *plausible*, they are not all equally so. A statement's *plausibility* increases in proportion as it approximates the more closely to being a statement of simple self-identity. The nearer a statement comes to being a tautology the more *plausible* it is. Statements of identity in time, therefore, since they of all synthetic propositions come the nearest to being statements of simple self-identity, possess a maximum *plausibility*. Thus by transforming an empirically discovered natural law into a statement of identity in time, we confer upon it a *plausibility* of the highest order.[28]

Scientific laws and principles are not, according to Meyerson, strictly necessary[29] since their necessity can be established neither *a priori* nor *a posteriori*. But they are *plausible*. They approach as near to simple self-identity as is consistent with "saving the phenomena." They gratify our desire for identity as far as is consistent with the facts of experience.

We are now in possession of Meyerson's answer to our question concerning the explanatory force of identity in time. The explanatory force of identification is due to the fact that this process, by transforming empirically discovered natural laws into statements of identity in time, makes these laws more *plausible*, that is more acceptable to reason, than they otherwise would be.

But is this answer satisfactory? Is Meyerson right in holding that scientific explanation consists in the attempt to reduce scientific laws to statements of identity in time thereby increasing their *plausibility*

[27] I.R. p. 161–162 and C.P. p. 618.

[28] "Toute proposition stipulant identité dans le temps nous paraît *a priori* revêtue d'un haut degré de probabilité" I.R. p. 162.

[29] "Aucun phénomène, même ce plus insignifiant, n'est complètement explicable." I.R. p. 459.

and thus making them more acceptable to our minds? It is, I think, plain that he is not. For if Meyerson is right, as he surely is, in holding that to explain a natural law is to exhibit its necessity then he must certainly be wrong in maintaining that to explain is to identify. For the reduction of scientific laws to statements of identity in time in no wise serves to exhibit their necessity since, as we have already seen, statements of identity in time are themselves contingent. Nor is this difficulty obviated by pointing out that statements of identity in time approximate statements of simple self-identity. For although it is obvious that statements of identity in time do, in certain respects, resemble statements of simple self-identity, yet it is nevertheless clear that they do not resemble them in the only respect that is in this connection relevant, namely in respect of their necessity. No matter how much statements of identity in time resemble tautologies in other respects, they differ from them in being contingent and are, therefore, if to explain is to exhibit necessity, as much in need of explanation as any other statement.

To explain a law is to show why it is true, but the fact that a law is a statement of identity in time gives us no reason for believin g thatthe law is true. For even though it be the case that the human mind has a marked preference for identity it is plain that nature does not share this preference. The fact that a law is a statement of identity in time taken in conjunction with the fact that the human mind has a marked preference for identity may, in some cases, be sufficient to explain why as a matter of fact we believe it to be true, but it clearly supplies us with no reason why we *ought* to believe it. The fact that a law is a statement of identity in time may be a cause of our believing the law, but it can never be a *rational cause*, that is a ground or reason, for our believing it. For a ground or reason must be not only a cause of the belief, (i.e. of the act of believing), but evidence for the truth of what is believed, and the mere fact that a proposition is a statement of identity in time constitutes, as we have just seen, no evidence whatever for its truth.

That the mind's preference for identity in time has played an enormous and hitherto largely unsuspected part in determining the growth and development of scientific theories Meyerson's magnificent researches in the history of science have conclusively established. But this result, interesting and significant as it unquestionably is, does not serve to establish the contention that explanation is identification. When we ask for the explanation of a natural law, we are asking not for the psychological causes which lead us to believe the law, but for

evidence which can assure us that the law is, and must be, true. The fact that a proposition is a statement of identity in time does not constitute such evidence. Hence it follows that identity in time is without explanatory force. And since it is without explanatory force, our preference for it cannot be other than irrational.

Is explanation identification? This is the question with which this paper has been concerned and to which an answer has now been given. Explanation is not identification. To explain is, as Meyerson has himself so ably shown, to exhibit necessity, and it is this precisely that the process of identification is powerless to do. The human mind no doubt possesses a preference for identity, but this preference is quite irrational and cannot, therefore, be equated with the mind's desire for explanation.

Princeton University.

Structure in Sentence and in Fact

BY

WILLIS MOORE

LUDWIG WITTGENSTEIN and certain of his friends and followers have offered an interesting answer to the question of the relation "one fact (such as a sentence) [must] have to another in order to be capable of being a symbol for that other".[1] The answer in brief, as Russell puts it, is that, "in order that a certain sentence should assert a certain fact there must . . . be something in common between the structure of the sentence and the structure of the fact".[2] Wittgenstein himself in the *Tractatus* usually denominates this common element as *logical structure*. The sentence is said to *picture* its fact; it *reflects*, *mirrors*, *shows* or *exhibits* by means of its own structure the structure of the fact it is about. Critics and interpreters generally agree with Russell's broad characterization of the theory but disagree greatly as to the exact nature of this postulated common structure. Consequently, the evaluation of the theory which this paper proposes to make must include an examination of its several alternative interpretations.

At least one logical positivist calls this common element *grammatical* structure. Mrs. S. K. Langer writes: "The sentence may denote any fact which has a structure similar to that which we call the syntax. If we know the grammatical structure, i.e., the relation which the parts of the sentence bear to each other, then we know that the possible denotations of the sentence as a whole are limited to those things which have an analogous structure".[3] Such passages as the following from the *Tractatus* might seem to support this interpretation: "One name stands for one thing, and another for another thing, and they are connected together, and the whole, like a living picture, presents the atomic

[1] Russell, Bertrand, *Introduction* to Wittgenstein's *Tractatus Logico-Philosophicus*, p. 8.
[2] *Ibid*, p. 8.
[3] *The Practice of Philosophy*, pp. 112–113.

fact".[4] "That the elements of the picture are combined with one another in a definite way, represents that the things are so combined with one another. This connection of the elements of the picture is called its structure. . . ."[5]

Both Russell[6] and Schlick[7] have cautioned against the confusion of what they call *grammatical* and *logical* structure. Mrs. Langer may be making some such mistake in interpreting Wittgenstein, but the view is worth examining anyway. The first question is as to the nature of *grammatical* structure. For the purposes of this analysis we shall take Russell's explanation offered in *Our Knowledge of the External World*.[8] That which remains constant in the series of sentences beginning with "Socrates drank the hemlock" and ending with "Coleridge ate opium" is what will serve as an example of the structure or form as distinguished from the content of a sentence. These sentences have in common a certain arrangement of certain types of words. Symbols for an actor, an act, and an object acted upon appear, as read or as spoken, in the order named. This structure we call, in grammatical terms, the active voice type.

Now, is it true that this structure is common to these sentences and the facts they express? First, can the objects meant by the symbols, namely, the actor, act, and object acted upon, be wholly or in part the element common to sentence and fact? They do appear in the fact but clearly do not appear in the sentence. The constituents of the sentence are *symbols* for actor, act, etc., but not the things symbolized. Furthermore, the *particular* actors, acts, etc., by definition are not parts of the structure; and how an actor or an act in general could appear either in sentence or in fact is incomprehensible to me.

A more likely supposition as to the nature of grammatical structure is that it is the configuration of the elements of the sentence, their different meanings serving merely to distinguish one from another. In the written sentence the order is spatial; as read off or spoken it is temporal. Can either of these be the structure said to be common to a sentence and its fact? Assuredly in some cases the spatial arrangement of the sentence elements *could* be similar to that of the fact. For example, in the sentence, "John sits next to James", we could be picturing the spatial

[4] *Op. cit.*, 4.031
[5] *Ibid*, 2.15.
[6] *Introduction to Mathematical Logic*, p. 199.
[7] University of California Lectures, 1931–2.
[8] Pp. 42 ff.

relationship of John to James, where James is to the right of John. But the obvious fact that the spatial arrangement of elements could be (and generally is) different in the sentence and its fact shows this factor to be an accidental feature of symbolization. The sentence, "Socrates drank the hemlock", is one such contrary example. Could Socrates in the fact be to the left of the act and the act to the left of the hemlock? To ask the question is to reveal the ridiculousness of the hypothesis.

Similarly, there is no *necessary* agreement between the temporal order of the elements of the sentence as read or spoken and the elements of the fact. Does Socrates in the fact appear first, followed by the act of drinking and the hemlock in the order named? Obviously neither spatial nor temporal configuration of sentence elements is necessarily a reflection of a like configuration in facts. This is not to deny the existence of such structures, or even the occurrence of types of such, in either sentence or fact. Nor is it to deny the possibility of either one of these occasionally directly reflecting what appears in fact. These resemblances, exemplified in our early written language are not logically necessary to expression or communication. The propositional sign, as a complex fact, necessarily has some structure or other, and man has learned to utilize this aspect of the sign to symbolize certain aspects of facts, even in some instances similar structural elements; but such symbolization is entirely arbitrary, within the bounds of the conventional system.

Our conclusion must be that if grammatical structure is wholly or in part spatial or temporal configuration of distinguishable sentence elements it cannot be the factor necessarily common to a sentence and what it is about. Any other interpretation of what is meant by grammatical structure would, I am sure, be subject to the same conclusion.

Wittgenstein in various passages in the *Tractatus* calls this common element the "form of representation", or "the logical form of representation".[9] "Form of representation" he defines as the possibility of structure".[10] What he seems to mean by this is that for one fact to picture another it must be able to exhibit the same mode of relation, e.g., a spatial fact can be represented by a spatial picture, a colored one by a colored picture, a logical one by a logical picture.[11] But, all pictures are also logical pictures and language is in terms of such.[12] There-

[9] *Op. cit.*, 2.17, 2.18, and 2.2.
[10] *Ibid*, 2.033 and 2.15.
[11] *Ibid*, 2.171.
[12] *Ibid*, 2.182.

fore, it would seem that every picture would have this necessary element in common with any fact. So, to say that a sentence has in common with what it pictures the possibility of logical structure is to say nothing of any significance; for what it has in common with all facts (including all other sentences) can in no way explain how it can represent some particular fact. Logical form, as the mere *possibility* of logical structure is not, then, the factor Wittgenstein intends.

The language of the *Tractatus* sometimes suggests that this factor is a "law of projection" or "rule of translation" for projecting or translating a fact from one medium to another.[13] In this rule or law, so it appears, lies the internal similarity holding between picture and pictured. But, if this be true this common element certainly is not, as elsewhere stated, reflected by or mirrored in a fact. The law itself is not *in* the fact or the sentence. Nor is this factor as usually described, such that it needs a law or rule to reveal it. Now there *is* a law of translation which enables a person to know what a propositional sign is about and a rule which a speaker or writer of one must follow if he expects to be understood; but these amount to no more than being conventional in the use of sensible objects as signs. Using these objects in communication as others do is difficult to construe as some element common to sentence and fact, or as one revealing such an element.

A more reasonable interpretation is the one given by Moritz Schlick.[14] He contended that for Wittgenstein logical structure has at least two aspects discernible in all sentences, namely, *multiplicity* and *order*. This interpretation is based upon numerous passages from the *Tractatus* of which the following are representative: "In the proposition there must be exactly as many things distinguishable as there are in the state of affairs, which it represents. They must both possess the same logical (mathematical) multiplicity".[15] "The propositional sign consists in the fact that its elements, the words, are combined in it in a definite way".[16] The first passage describes *multiplicity*; the second *order* or configuration.

It is significant that Wittgenstein illustrated what he took the relation between sentence and fact to be by means of an analogy with that holding between a line on a phonograph record and the stream of sound it records.[17] In what sense might this line and its stream of sound

[13] *Ibid*, 4.014.
[14] University of California Lectures, 1931–2.
[15] *Op. cit.*, 4.04.
[16] *Ibid*, 3.14.
[17] *Ibid*, 4.0141.

exhibit the same multiplicity and order of elements? We might at first perceive no similarity. One of the states of affairs concerned is a fine engraved line of varying patterns of indentation appearing on a dark disk; the other is a stream of tones varying in pitch, intensity, color, etc. More careful examination, however, reveals common features. If the recording is a good one the record will show a perceptible variation in these marks for every perceptible variation in the stream of sound. The two states of affairs exhibit the same number of variations, i.e., have the same logical multiplicity. Furthermore, they also show identical patterns of variation—for example, a deep indentation followed by two shallower ones on the record corresponds to an intense tone followed by two less intense ones in the stream of sound. So, in a fairly simple sense, the line on the record and the sound it records do exhibit an identity of structure. In this case the common element is sensibly discernible.

This description of the relation holding between a line on a record and the sound recorded is, I believe, accurate. If logical structure consists in multiplicity and configuration of distinguishable elements, then the line and the sound certainly have them in common. The problem lies, however, in the application of the analogy. Is the *sentence-fact* relation the same as the *phonograph record-sound recorded* relation? Wittgenstein assumes that it is.

We can deal briefly with the aspect of logical structure called *configuration* or *order*. If by such is meant the sort of thing just described as an arrangement of distinguishable elements in line and sound then we have already rejected it as a necessary common element as between sentence and fact. In the line and sound it is merely spatial and temporal configuration of elements, the same thing dealt with above as a possible interpretation of *grammatical* structure. What was said there holds here of *logical* order. Such spatial or temporal configuration as appears in a sentence is either non-significant or simply a conventional device for indicating something about a fact. What *is* thus indicated, whether or not it be a similar structure in the fact, could as well be indicated by some different configuration of elements, or even by a word or phrase.

Logical multiplicity, however, cannot be so easily disposed of. We must return to our typical sentence, "Socrates drank the hemlock", and re-examine it and the fact it represents to settle the question this analogy raises. Does this sentence have the same logical multiplicity as its fact exhibits? First, what *is* the multiplicity of the sentence, i.e., what is the number of distinguishable elements that compose it? This

question is not easy to answer. Must we count the letters? The dozens of variations in the marks lower in level than letters? Or if we ascend to the level of words, do we count the word *the*? Does the tense of the verb count? Or the direction of the act, however indicated? But let us pass over these difficulties, assuming for simplicity's sake that there are three elements present in the sentence, a symbol for actor, one for act, and one for object acted upon. Turning now to the scene pictured by the sentence—do we find there this same multiplicity of elements? The answer will depend largely upon what we take to be the fact represented. The scene I imagine as a result of reading this sentence is much more complex than the sentence. For example, I cannot imagine it without thinking some sort of container for the hemlock. If this scene suggested (which is probably no more complex than the scene in the presence of which the proposition was originally conceived) is what is meant by the fact represented by the sentence, then their multiplicity is not identical. The sentence is always the result of a narrow selection from a complex fact. Both the borders of the fact and the number of elements within those borders seem to be relative to the purpose and other factors in the mind of the perceiver of the moment.

If, however, by the fact represented is meant only those elements of the richer context strictly indicated (denoted) in the sentence, then by sheer definition the sentence and its fact have the same multiplicity. But such an identity of structure would not be analogous to that present in a phonograph record line and its sound. In this latter case a perceiver can read the multiplicity of the sound from the line and that of the line from the sound. The same is not true of the sentence and its fact. One cannot read the multiplicity of the sentence from its fact but only that of the fact from the sentence. In a sense the line does *mirror* the multiplicity of the sound, and vice versa; but the sentence can hardly be said to do so with regard to the multiplicity of its fact. Rather it *defines* this multiplicity. An analogy better suited to the sentence-fact situation, as here interpreted, than that of the phonograph record-sound recorded type is that of the use of lights in a theatre so arranged and operated as to throw into relief on the stage first one set of figures and then another at the will of the person controlling the lights. The lights correspond to the sentence and the figures on the stage to the fact expressed by the sentence. The sentence is, thus, in a sense, the creator of the multiplicity of its fact, not a reflection of it. At any rate, I think we must conclude that, on either interpretation of what is meant by the fact represented, the sentence-fact relation is not analogous to the one used by Wittgenstein for purposes of illustration.

But, for the sake of the argument, let us assume all that Wittgenstein claims here, that the picture and the pictured have a definite and an identical logical multiplicity. Even so, would this knowledge alone enable us to understand why this picture pictures this particular fact rather than some other? Certainly not. Two sentences could have exactly the same multiplicity of elements and yet represent two facts which differ in every respect except this one, that they, too, have the same number of distinguishable elements. Conversely, two facts with identical multiplicity, if differing at all in content must be represented by two sentences rather than one. Clearly, the phenomenon of representation by sentences is not explained by a mirroring of structure if by such is meant multiplicity of distinguishable elements.

Those who support this theory would probably contend that identity of multiplicity alone does not explain representation but that this together with order can do so. Unless by *order* is meant something other than configuration of elements this contention has already been dealt with. It is difficult to see how order and multiplicity taken together constitute the element common to sentence and fact if actually order is seldom so found, and never necessarily so, and, further, if multiplicity, instead of being common to the two, is rather *defined* in the one by the other.

What must we conclude as to this theory? If a sentence did represent its fact as a line on a record records its sound, we probably should have to agree with Wittgenstein's contention. But the situations are only loosely analogous. The line on the record bears a mechanical, causal relation to its sound. Man produces the record by taking advantage of concomitant variations in natural processes. Two facts that vary together thus *do* have an "internal similarity" of the sort postulated by Wittgenstein. Sentences, on the other hand, bear a different relation to their facts. The relation is not *internal* but rather one determined by a factor external to the two facts concerned. This third factor is the mind for which the two facts are associated. The sentence *means* its fact; the line on the disk *records* its sound. By virtue of *meaning*, any element of one fact can come to stand for, and represent, any element of another fact, even including its structure. It is not necessary for one fact to reproduce or exhibit the structure of another in order for a mind to see the one through the other.

There *is* a valuable aspect to Wittgenstein's theory and that is its stress upon the necessity for complexity in a sign that is to represent, rather than simply to denote a complex fact. Representation, so the view holds, differs from denotation. The former can present new facts,

by means of rearrangement of old elements, while the latter can only indicate the old. The possibility of complexity of a sign allows for a new dimension in language above simple denotation. But I doubt that this aspect of the theory is new. It is probably as old as Aristotle.

A final doubt must always confront a critic. Can he be sure that he has properly interpreted the theory criticized? It is possible, in this instance, that by *logical structure* Wittgenstein meant something not even hinted at in the paper. If such be the case then these conclusions probably miss their intended mark; but this is a risk men universally must accept when they turn critics.

University of Missouri,
Columbia, Missouri.

Conceptual Implications of an Indian Language

BY

D. DEMETRACOPOULOU LEE

IT HAS been said that a language will delineate and limit the logical concepts of the individual who speaks it. Conversely, a language is an organ for the expression of thought, of concepts and principles of classification. True enough, the thought of the individual must run along its grooves; but these grooves, themselves, are a heritage from individuals who laid them down in an unconscious effort to express their attitude toward the world. Grammar contains in crystallized form the accumulated and accumulating experience, the Weltanschauung of a people.

The study which I propose to present below, is an attempt to understand, through a study of grammar, the unformulated philosophy of the Wintu tribe of California.

I

The Wintu speaker divides his predicative statements into two categories: that of the subjective or experiential, and that of the objective. He has two ablaut stems[1] to choose from, for every verb. He uses one stem to express a state or action in bringing which about the grammatical subject can or does participate, another for the state or action that is irrespective of the agency of the subject. Correlatively, the speaker himself participates in the event or state of the first category (I), in so far as he perceives or apprehends it; but that of the second category (II), is the object of his belief: in speaking of it, he asserts a truth which is beyond experience.

[1] For example, for the verb *to come* there is the pair of stems *wir-* and *wer-; to swallow* has the stems *pira-, peru-; to wash: yuqa-, yoqu-; to chop: kup-, kop-.*

Category I is the category of subjectivity, and expresses this subjectivity through various mechanisms. The form of the stem denotes experience on the part of the grammatical subject. A long series of suffixes show cognizance of this experience on the part of the speaker; and a small group of modal suffixes indicate the attitude of the experiencing subject toward his experience, actual or projected.

The statements of this category, then, express only partial truth; that is, truth as limited through the sensory and mental apparatus of the knowing subject. So patent is this limitation, that the language provides suffixes expressive of the exact channel through which this particle of truth has become known. Each of these denotes, not only person, time and aspect, but also the manner in which the speaker himself has come by his information. The speaker can use the stem alone, without suffix, thus stating generally known fact. But if he tries to particularize this, to delimit it as to time or subject, he is forced to quote, in the same breath, that particular experience of his which is his authority for the statement. He cannot say simply, *the salmon is good.* That part of *is good* which implies the tense (now) and the person (it), further has to contain one of the following implications: (the salmon is good) I see, I taste (or know through some sense other than sight), I infer, I judge, I am told.

If the information given depends on hearsay, the suffix *-ke*,[2] expressive of all three persons, is attached to the stem. It is the suffix which is used exclusively in the narration of myths; also, in gossip, in descriptions of places which the speaker has not visited, etc. It is really the suffix of the third person, as there are seldom any occasions when it can be used in describing an event pertaining to the first or second persons. If the occasion calls for it, however, it is used in reference to these persons also. The following two sentences illustrate such use:

tsoyila*ke* ni: I *am* drunk, (*I hear*); they tell me I'm drunk.

kilepma kuyabi*ke* mi: frightfully sick you *are*, (*I hear*); you are supposed to be very sick.

Visual evidence is expressed by means of *-da*, *-besken*, *-be* for the first, second and third persons respectively. The visual is the only evidence which is distinguished by means of a separate suffix. Primarily, this suffix seems to indicate unquestioned evidence. It is used in purely exclamatory, non-informative phrases. Apparently, it comes to refer to visual evidence only incidentally, only because through the eyes alone

[2] The phonetics have been simplified throughout, in conformity with the non-technical nature of this paper.

comes evidence which can be used as data without question. As a rule, then, this suffix indicates supremely reliable evidence; it usually denotes visual evidence only when emphasizing the contrast to evidence derived from other senses. This is especially true of the first person, since in this case the speaker is one with the directly experiencing grammatical subject. *harada, muteda, wineda, hureibida:* I am walking, I hear, I see, I am sewing, tell their own story. The following phrases illustrate the use of the suffixes for the second and third persons: mi yo tchala*be*: that *is* a beautiful tree (*I see*) kenpana sanih*abe*: get up, it *is* morning (*I see*); tchepkala*besken*: *you are* in a bad state (*I see*); *you look* rotten. In this last, the suffix indicates specifically visual evidence; otherwise, say the Wintu, the statement is worthless, since it tells nothing which the addressee does not know.

When a statement is based on sensory evidence other than that of vision, the suffixes *-ntida, -nteresken, -nte* are used for the first, second and third persons respectively. hire*nte* qewel: the house *is* burning, says the blind man, who smells the smoke and feels the heat; tubelabi*nte*: it *is* fragrant; pilamabi*nte*: it *is* warm; pe nis iya*nteresken*: why, you *are* biting me! is said to an invisible insect: poyok kuyabi*ntida*: I *am* aching as to the head; I have a headache; una*nteresken*: you thus-ed (*to my hearing*); thus you said.

When a statement is based on inference, the suffix *-re* is used for all three persons. This suffix indicates that logic has been applied to circumstantial sensory evidence. Naturally, it refers chiefly to the third person, though it is occasionally used in statements about the first and second persons. ewin minelpukeki*r(e)*kelesken: by means of this you almost died, (*I diagnose*), says the shaman, holding up the extracted spirit-arrow; nitchai yo hara*re* nor: my nephew *is* gone south (*I infer*); my nephew must have gone south, says Coyote. He sees Hummingbird's tracks ending suddenly, he sees that the valley to the south is covered with gay flowers, and infers that Hummingbird has flown south. The necessity here, which I render as *must*, lies in logic, not in nature. The Wintu does not confuse the two.

When his statement is based on conclusions derived from systematic thinking alone, or on judgement, the Wintu uses the suffix *-el*. Often the *-el* can be rendered into English only with the aid of *must*. As the preceding suffix, this refers to logical necessity. pite bera*el*, my father-in-law *is* hungry, is said by a man who knows that his father-in-law is bed-ridden and alone. When Coyote, who has decreed that there should be death in the world, loses his son, Fly says to him, tchala*el* mi

man watchasuker: it *is* just (*I judge*) that you weep. har walel yowuna*el*: go, you two, he *is* homesick for you, says a father to his visiting daughters when he is sending them back to their husband. He has not seen the husband, nor has he heard news of him; but he knows that the man loves his wives, and bases his statement on a logical deduction from this known fact.

To summarize the functions of these five suffixes, I shall use a paradigm. If I want to translate the sentence, "Harry is chopping wood" into Wintu, I have a choice of the five following sentences, according to my source of knowledge:

Harry kupake, if I know this by hearsay.
Harry kupabe, if I see or have seen Harry chopping.
Harry kupante, if I hear him, or if a chip flies off and hits me.
Harry kupare, if I have gone to his cabin to find him absent and his axe gone.
Harry kupael, if I know that Harry has a job chopping wood every day at this hour, that he is a dependable employee, and, perhaps, that he is not in his cabin.

These suffixes play an important part in the verbal structure of the Wintu. With the aid of other affixes, they are used of past time, of the subjective future, of the durative, the habitual, the responsive; in short, with moods and tenses used in describing experience. Even questions have to be couched in such a way as to elicit information derived through a particular medium. For example, the interrogative *-bewi* assumes that the questionnee has been an eye-witness. hestit ie*bewi*: what sort of a person *is she*, contains the implication: you have seen her. The *-nteri* shows the expectation of auditory evidence from the questionnee. watcha*nteri*: did *she* cry? (*did you hear her* cry?) The interrogative correlative of the *-ke* of hearsay is *-boh*. My informant, who could not keep pace with European politics, once asked me: manapuribia*boh* popom: *are they* still fighting (*according to the newspapers*)?

The distinctions made by the five suffixes so far given correspond to subjective differences in the speaker, not the grammatical subject. Other affixes, added to the stem and preceding any personal or temporal suffixes there may be, indicate differences of attitude on the part of the grammatical subject. These are: *-wira*, and *-puke*. Both of these occur as independent stems, and, in their suffixed form, may be said to be reminiscent of their meaning when independent.

-wira, when an independent stem, means *to come*. When a suffix, it implies futurity which is dependent on the desire or intention of the

grammatical subject. Primarily, it is used to bind together two events or states, whose mutual relationship is that of sequence, a sequence brought, or to be brought about, through the agency of the individual. Both of the two events may be in the past or in the future; or one may be present and the other future. The suffix concerns itself, not with their temporal relationship to the speaking subject, but with the sequential relationship of each to the other. Thus it may be translated, according to context, variously as: *to, so as to, before, with the expectation to, about to;* and, followed by the subjective suffixes described above, it forms a future tense, which the Wintu render into English as *shall* or *will.* But, in every case, it is to be kept in mind that, implied in the *-wira* itself as well as the subjective stem to which it is attached, is the notion that the succession of the two events has been or is being brought about by the sentient, intending individual. To illustrate, I quote a few sentences from texts:

hina*wira*biar: being *about to* arrive.

yuptcha*wira*kila: when they were *about to* shoot.

hlaqal*wira* hinada: *so as to* play I have arrived; I have come *to* play.

ba*wira* we're: *to* eat come; come and eat.

Each of the last two sentences implies planned succession. In translation, either the planning or the succession is rendered, according to which meaning seems to predominate. In a phrase such as the following, it is difficult to make the choice: huntchiyekilake[3] nopum ba*wira*: they invited him, it is said, *before* they ate the deer; or: they invited him *to* eat the deer. The Wintu hearer is disturbed by no ambiguity in such a statement. The distinction which is of importance to him—that of voluntarily determined succession versus necessary sequence—has been made.

winyupe*wira*ibe: they *will* assemble; they *plan to* assemble.

hara*wira*ibida: I *shall* go.

-puke, as an independent stem, means *to be not quite cooked; to be partly raw.* As a suffix, it indicates approximation, not quite realized experience, whether past, present, or future. In English, it is necessarily rendered as *almost, like*, or with the aid of the irrealis *would.* It must be noted here that *like* refers to different degrees in the realization of the same state; it compares the almost realized to the completely realized.

hitchuna*puke*da semum: I *almost* cut my finger.

[3] The *-kilake* (hearsay knowledge of the distant past) is affixed to all verbal phrases coming from myths. In translating, I shall give only the past tense and shall not add the implied, "it is said."

hara*puke*da: I *came near* going; I *almost* went.

qotisa*puke*besken: strong-*like*-you-are (I see); you look *as if you would prove to be* strong.

I shall mention one more subjective suffix, which, however, is attached not to stem I, but to the nominal stem of the verb. Like *-wira*, it occurs either without accompanying suffixes, or with the subjective suffixes. As an independent stem, *kuya* means *to ache, to ail.* As a suffix, it means *to desire, to try.*[4] Like *-wira* and *-puke*, it does not express a temporal relation to the present in which the speaker makes his statement; even the temporality contained in its relation to other events is only incidental.

hiina*ku*(*ya*)da: I *want* to sleep; tlomi*koyu* (ablaut form of *-kuya*): *try* to kill him.

To summarize: In Category I the form of the stem implies that the grammatical subject participates or has participated as a free agent in the activity or state described by the verb. The grammatical subject may be the same as the speaker, as in the first person, or may be different, as in the second and third person. In either case, the speaker participates himself, insofar as he has become cognizant of the activity or state described. Further, we have the attitude of the grammatical subject toward a contemplated participation; his attitude may be that of intent or purpose, (*-wira*), of desire or effort, (*-kuya*), or merely a reflection on its feasibility without necessarily a determination to carry it out (*-puke*). Lastly, we have the position of the speaker toward his own statement, indicating the exact limitations in the validity of the known fact to which he gives expression.

II

The stem of Category II is essentially passive, though it is used for many forms which we do not recognize as passive in English. It is used in the formation of the passive and mediopassive. It forms the imperative. It serves to pose questions whose answers do not depend on knowledge on the part of the speaker or hearer, and to make wishes of the day-dream type. The suffix of ignorance, used variously to express negation, interrogation, or wonder, is affixed to this stem. Furthermore, to this stem of Category II is attached a suffix whose underlying meaning seems to be that of natural necessity and which corresponds to

[4] This relationship between aching and desiring is present also in colloquial English. We say that we are "aching to do it"; we are "dying for a ride"; we "crave companionship."

the modal suffixes of Category I. This suffix is used to express, all in one, futurity, causality, potentiality, probability, necessity; to refer to an inevitable future which might, can and must be, in the face of which the individual is helpless. Category II has reference to a state of being in which the individual is not a free agent.

In the statements of this Category, attention is concentrated on the event and its ramifications, not on the actor. The verb is not particularized in terms of participation. There are rarely any personal suffixes, and the speaker never refers to himself. He is not an authority. He speaks of the unknown, and when he makes his assertion, he asserts truth which is subject neither to experience, doubt or proof.

For the formation of the imperative proper, that is, for the issuance of a command to the hearer, stem II is used alone. Allied closely to this, and felt to be merely the first person of the imperative, is the command one gives to oneself—exhortative. In the expression of this, we have a breakdown of the rigidity in the distinction between subjectivity and objectivity. Either *-wira* of intention or *-le* of necessity may be suffixed to stem II, apparently according to no principle, but rather according to momentary preference. However, the breakdown is only partial; the *-wira* itself appears in stem II, as *-we(r)*. There is some reason to believe that the *-le* in this connection is going generally out of use. It occurs in old songs, and in stylized conversation in one myth. Only in the inclusive plural is the *-le* commonly in use now; i.e., only when the speaker includes those spoken to with himself in his exhortation, an occasion where a strong element of command is necessarily present.

To express an idle wish, which he does nothing to further, the speaker uses stem II with the suffix *-di* or *-u*. I quote a few such wishes to illustrate:

witil sanih*adi* po: *may it* soon be daylight!
ewin be*di* po: *would that* he were here!
olelas po ni be*di*: *would that* I were tall!

The *-u* may form either an optative or a question, and its specific function can be understood only by the context.

tera di*lu*: (at his feet) gliding down may I fall!
hekenbom weril*u* paqam: from where shall we bring manzanita wood?
heken pele pominilheme*u*: where shall we set him down?

The two latter sentences could have been translated as optatives, as

heken also means *somewhere;* for instance: from somewhere may we bring manzanita wood!

Stem II is further used in the formation of negative statement. May we take this to mean that the Wintu feels that not-being cannot be known? The function of the *-mina*, which helps form the negative, seems to be to emphasize the ignorance involved in the use of stem II. It takes assertiveness from the verb, and makes the verbal phrase contain, in disjunction, both affirmative and negative meaning. It is only the auxiliary words used in the sentence which give it specific meaning. In the sentence, it comes to mean negation, interrogation, or merely a sense of wonder over the unknown.

elebe nis nop minelpaqas koyu*mina*: not-do[5] for me deer die-for want-wantnot; deer do not want to die for me. (*-koyu* is stem II of the suffix *-kuya*: to want).

eleukilake peh tin*mina*: not-did anything say-saynot; he did not say anything.

hadi henulebo Harry were*mina*: unknown whether-shall (actually *some-alternative-shall*) Harry bring-bringnot; I wonder whether Harry will bring it.

hestam nurum weril*mina*: which-did-you (or: how-did-you) salmon bring-bringnot? did you bring the salmon?

As already mentioned, there is only one suffix, *-le*, corresponding to the three modal suffixes in group I. It denotes the sequential relationship between events, a relationship which is, apparently, inevitable. It is a contracted form of stem II of the independent verb *lila*. This verb, in stem I, means *to make, to become;* in stem II it might be said to carry the connotation: *to become perforce.*

The *-le* takes a suffix when it refers to what we would call future, that is, a condition which is future to the time in which the statement is made. The suffix is formed from contracted forms of *buha*: *to be, to dwell,* and probably contains a durative element. It is differentiated according to the three persons. The futurity referred to is a necessary futurity. According to context, one or the other of these concepts—necessity and futurity—is uppermost, though the other is always present. I quote phrases from texts:

mia man minel (*l*)*ebosken*: you too *shall* die.

mi uniharakila qotisa*lebosken*: when you get to be as old as I am *you are bound to* be strong.

[5] I translate *eleu* as *to not-do*, or *to not-be;* actually, it means: *to not.* It is not a negative statement, but, rather, a positive assertion of negation.

There is an instance where a bird is instructing two boys how to kill a flying monster. "She comes flying in the evening," says the bird, "and her wings make a terrific noise. mut*lebosken* lel weret: *you shall* hear her come (*you can't help it*)." There is no element of intention in this stated future. I have recorded a short love-song which illustrates this type of futurity. A man and a woman, lovers, are about to part finally. They sing: pelen tosmaliton be*lebom*: the sleeping-place which you and I hollowed out *shall* remain. The Wintu explain that this really means: you and I can put an end to our relations; but we cannot obliterate the place on the hill where we have slept. It is that *-lebom, it shall, despite ourselves*, which gives poignancy to the song.

In the examples given so far, the element of futurity was predominant in the *-le*. In the following illustrations the concept of necessity lies uppermost.

har walel be*lebada:* go, you two, I must stay (says the bedridden man).

In one of the myths, when the hunter sends Grasshopper to Coyote with a message giving him directions as to how to procure food magically, Coyote is in bed, sick with starvation, and knows nothing of the hunter's kind intentions. The hunter says to the messenger: hon*lebom* baleso: *he shall* get dry before he eats. This is equivalent to saying: he must get dry before he eats. He shall because he must; he is starving, he knows what the conditions for securing food are, and he shall fulfill them. But he has no intention of doing so, as he knows nothing of this yet. Compare such a future, with the future expressed in Coyote's petulant threat to Grasshopper, who is biting him on the back by way of announcing his presence: ba*wira*ibida mis: I shall eat you. Even English recognizes the weakness of such a future and renders it idiomatically as *I have a mind to eat you.*

Concepts of potentiality, obligation and probability are undifferentiated morphologically. They are expressed by means of *-les*, the nominalized form of *-le*. In translation, only, they have to be distinguished because of the necessary use of distinct auxiliaries in English. In the following examples, the person is given by separate personal pronouns (ni, mi), or not at all.

nurpaq ni hawal(*l*)*es*: I *would* choke on a salmon bone (if, or when, I went to the McCloud).

nanname*les* mi: you *must* tell the truth.

ha*les* mi: you *must* go.

minelkila tchalu*les*: if he died it *would* be good; or: when he dies, it *shall* be good.

henu*les* ni: how *can* I (I wonder).

ni kute man tipe*les*: I am *able to* send off few sparks.

In the negative phrase, the concept of capacity comes to be distinguished from that of obligation and probability. This separation is possible because the negation can apply to the potential event itself, or to the potentiality of the event. It is the difference between saying: the next step is not-potential, and: the next step is-not potential. The first then comes to be reserved for the expression, "I cannot because I must not"; or: "I would not because I must not"; the second for, "I cannot because I lack the capacity."

To express negative obligation the *-les* is affixed to the negative auxiliary *ele(u)* which means *to not(do)*, *to not(be)*.

ele*les* ni yoqumina: I *must* not wash; or: I *can*not wash (*because I am not allowed to*).

ele*les* mi harmina kilepmabinte: you can*not* go, it is frightful; i.e. you must not go.

To indicate lack of capacity *-pina, to lack.* is affixed to *-les*.

kenpana*lespina*kilake kiye: Coyote *could not* get up; (he was very old).

baskenperu*lespina*da: I *cannot* swallow food; (it would stick in my throat; my parents just died).

qayu*lespina*da: I *cannot* travel.

Due to the generic meaning of *-les* there can be overlapping of the functions of *eleles* and *-lespina*. The sharp division, however, is to be found between the "cannot" which has to be accepted because nothing can be done about it, and that which can be repaired. There is no suffix to indicate inability due to some purely subjective and reparable cause; but the verb *iwiya* is used, when incapacity is due to ignorance.

hur*lespina*da: I *cannot* sew (*because I am blind*); but:

hurus *iwiya*da: I *cannot* sew (*because I don't know how*).

Without a personal suffix, the *-le*, as rendered into English, is necessarily indistinguishable from *-wira*, due to the limitations of the English language. Like *-wira*, it is translated as: *before, so as to, about to.* Actually, the *-wira* implies free agency and intention, whereas the *-le* does not.

When the *-le* means *about to*, an auxiliary *nuya* is often used in the

sentence. When the *-le* means specifically *before*, it is sometimes followed by the suffix *-so*.

sanih*a*le nuyakilake: it was (*perforce*) *about to* be morning.

kenwanu*le* nuyakilake: it was (*perforce*) *about to* be evening.

kiyetchepet minel(*l*)*e* nuyakilake: poor old Coyote was (*perforce*) *about to* die.

ha(r)*le* nuyada: I am (*perforce*) *about to* go: I *must* go.

1. [6] honlebo ba*leso*: he shall get dry, being (*perforce*) *about to* eat; i.e. he shall get dry *before* he eats. This excerpt from the texts was given earlier in connection with the future. It is clear that the eating is not intended by the grammatical subject as he knows nothing about it at the time this statement is made. The *perforce* in this case would have reference to the fact that the future is coming despite Coyote, the eventual eater.

   tintinpurebuhakilake hiina*leso*: they sat talking, being (*perforce*) *about to* sleep; in English idiom: they sat talking *before* they went to bed. They went to bed by force of custom, as a matter of course; talking beforehand was a matter of momentary preference.

   tchus tlitiq*leso* setlamahnabuhakilake: (*perforce*) *about to* gather wood, he sat thinking; before he gathered wood he sat thinking. Gathering wood was a daily duty.

2. yole yoqu*le* tchawuwen: (*perforce*) *about to* wash, let me sing a while; let me sing a while before I wash.

3. yoruna*le* yole tlitiqmina sukeda: (*perforce*) *about to* work for wages, I stand a while working on nothing; I am doing nothing for a while *before* I go to my job. (I have to go to my job, but I can do what I like before I go).

4. harebo hlosu*le*: *about to* launder (*perforce*), let us go; or: let us go *so that I may* launder; or: let us go *before* I launder. Actually, when my informant used this, she meant *so that I may launder*.

The *-le* and *-leso* refer only to the first and third person. The corresponding suffixes for the second person are *-men* and *-menso*. There is no particular significance in this change of suffix. There is reason to suppose that the introduction of the use of a second person in the verbal phrase of the Wintu has come after the development of the other two persons.

5. waiken har*menso* . . . yelwinit: *before you* go over the brow of the hill . . . look back at me!

[6] Some of the following examples have been numbered so as to facilitate subsequent reference.

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6. kenwawu tepu*men*: open your mouth wide *so that you can* get across.
7. hul*men* boi ba: eat much *so as to* get fat.

hima har*men* lel pominpana: *before you* leave tomorrow morning go to bed. This is the translation of my informant. We would have said: go to bed *since you must* leave tomorrow morning.

8. we'e war sehosuna*men*: come *to* seek luck!

When we look over the preceding examples, it becomes clear that the necessity in the sequence may lie either in that the indicated event is inevitable, or merely in the relation between the two events. In either case, the phrase shows an interplay between free will and necessity. Event B is fixed; there cannot be an AB sequence unless event A occurs; but event A is in the power of the individual. Such, I take it, is the meaning of examples 2, 3, 5. Apparently we can also have the following situation: there is a sequence AB; neither A nor B need come about; but if A comes, B must come; so, if you want B, you must bring about A. If you want to get across without drowning, open your mouth wide, (says the wily old man to the pursuing bear). If you want to get fat, you have to eat much. So, in examples 4, 6, 7, 8, the necessity seems to exist in the first event, though, actually, this need not take place except at the wish of the individual. We should have called this a causal sequence, if it were not that causality is simply one of the concepts read by English-speaking people into the broadly sequential *-le*.

To give a complete description of this latter function of *-le*, it would be well to mention another suffix whose function is the reverse of this. Like *-le*, this suffix points to an AB sequence, but contrary to *-le*, it stresses the undesirability of the realization of B. It indicates that a dreaded event hangs upon the fulfillment of a preceding event; if event A does not come to pass, event B will be prevented from coming; or, if event C is substituted for A, B will be avoided. Instead of the implication of *-le*: do, so that you may, it has the meaning: do not, so that you may not, or: do otherwise, so that you may not. Thus it comes to be translated variously as *might*, or *don't*. I found it impossible to understand it, till the meaning of *-le* and *-men* became clear. The suffix is differentiated according to person; *-kida* for the first, *-ken* for the second and *-kedi* for the third. Examples follow:

elewen dumtchaharmina; toptchuher*kida* tlaqun: let me not go to bathe; *I might* get stung by a rattlesnake; or: *let me not* get stung by a rattlesnake.

tchaluma ih tal*ken*: look out, *you might* fall! or: look out, *don't* fall!

tchaluma winebiar yuptchu; mana*ken*: look carefully when you shoot; *don't* miss.

pia ih*kedi*: he *might* do it himself; or: *don't* let him do it (do it yourself).

-kilili tchinu*kedi*: (if you were to let her come) she *might* get the bulbs; or: *don't* let her get the bulbs.

It is impossible to do justice to this suffix in translation, as my informant often complained.

The *-le*, then, with its variants, is used for referring not to events, but to the relationship between events. Stem II, itself, implies lack of knowledge. The suffixes of the imperative, optative, negative etc., necessarily do not refer to present actuality. In short, Category II refers either to helplessness in face of events, or to ignorance, or to a future which, from a stated point of vantage, has not been realized. In this sense, Category II does not correspond to Category I, as it does not refer to that which is, contemporaneously with the present of the speaker, or of the grammatical subject. Yet there is a way in which Category II can be used to make, circuitously, a statement about the present. It does not describe this, but, so to speak, points to the ineffable. To do this, the suffix *-les* is affixed to stem II of the verb of being (*u-*) and is followed by *uni*, which means *thus*, forming *ulesuni: to-be-thus*. The Wintu sometimes translate this as *almost*, thus making it indistinguishable from *-puke*. But, *-puke* refers to an approximation, a stage in the process of becoming, whereas *ulesuni* refers only to a similarity between two disparate situations. It might be said to indicate appearance only. More often, the Wintu translate it as *like*, and as *like* do I translate it, ungrammatically, in the illustrations given below. The *ulesuni* is then used in a verbal phrase where the verb is in Category I and with suffixes of experience. We take it that the speaker likens something unknown to something within his experience. The burden of proof is left, then, to the event itself, not to the speaker; or, the speaker gets out of all responsibility for the truth or falsity of his statement, by implying that something has the appearance, only, of being such-and-such, so far as he knows.[7]

[7] The connection between similarity and appearance is thoroughgoing in the Wintu language. The usual word for *like*, or a *similar one* -oqti- means etymologically: surface-of-the-identical. It has been formed from *uqa, oq-:* identical, and *-ti:* side of, surface of, and might be taken to mean: the appearance of the identical.

pom oltikal*ulesuni*kilake: the land flashed-*like;* (a metaphor; the land was covered with bright flame-colored poppies).

seponora*ulesuni*kilake: he was running about *like; it seemed as if* he was running about.

tuteliqta*ulesuni*kilake: took him in (into her mouth) *as it were* with one lick.

heken kaha*ulesuni*kilake: somewhere there-was-blowing-*like;* there was something like wind.

tlitiq*ulesuni*besken: you work-*like* (I see); you seem to be working.

III

To conclude: The Wintu has a small sphere wherein he can choose and do, can feel and think and make decisions. Cutting through this and circumscribing it, is the world of natural necessity wherein all things that are potential and probable are also inevitable, wherein existence is unknowable and ineffable. This world the Wintu does not know, but he believes in it without question; such belief he does not tender even to the supernatural, which is within experience and can be referred to by means of *-nte* (I sense).

The Wintu are not conscious of their own distinctions, or of their own Weltanschauung. But, obviously, their coherent morphological system is no accident, and must have been created throughout the years because of their unconscious attitude toward the world without and within them. It is doubtful that the Wintu developed by themselves the various mechanisms by which they express this Weltanschauung. The verbal ablaut is found in related and unrelated languages among surrounding tribes; but there its function is either not regular or is entirely different from its function in Wintu. Suffixes giving the source of information are found in unrelated languages of neighboring tribes to the north. The Wintu may have borrowed ablaut and informational suffixes from their neighbors or may have contributed them. They undoubtedly originated themselves the idea of combining the concepts given above under the one suffix *-le*. But the question of origins is non-essential. Whether they invented, or used material at hand, they have integrated a number of discrete grammatical phenomena into one consistent morphological system, to express their fundamental categories: subjectivity versus objectivity, knowledge versus belief, freedom versus natural necessity.

Pomona College,
Claremont, California.

DISCUSSION (CORRESPONDENCE)

THE CONCEPTION OF INFINITE PROGRESS

Sir:

May I comment briefly on the difficulties found by Professor Lovejoy (*The Great Chain of Being*, pp. 330f.) in the doctrine of the infinity of the past temporal process?

(1) "The paradox of a completed and told-off infinite sequence of events." This seems to presuppose a definition of the infinite as incapable of completion—a definition which, although manifestly inadequate, may be accepted for the sake of this discussion, since it is a corollary of the idea of one-to-one correspondence of whole and part now regarded by mathematicians as essential to the conception of an infinite collection.

We must ask, however, what is meant by saying that a sequence is incapable of completion? All that is involved, it seems to me, is (a) that there is no last term, or (b), that if in imagination we retrace the series of terms we never come to a first term. Whether a sequence which is incapable of completion can be "told-off" depends upon our interpretation of this phrase. If the expression means simply, that the units included in the sequence are to be added successively one by one, *without the assumption of a definite starting-point*, there is no difficulty; for we need not suppose this process of addition to be finite. Even if a starting-point is assumed, but is placed somewhere along the course of the sequence, there is again no difficulty; for whether the telling of units then proceeds forward or backward in time, it may be incapable of completion. Only if it is arbitrarily decided that the telling must start with a *first* event does a "paradox" arise; but, such an arbitrary decision would obviously be a begging of the question at issue.

(2) "The supposition . . . of an infinite number of changes through which no commensurate realization of value seems to have been attained." The question here turns upon the interpretation of the adjective "commensurate." The word suggests some sort of ratio or proportionality between time elapsed and work performed. But, before such a correlation can be significantly affirmed or denied there must be a

basis of comparison. Shall we say that nothing less than an infinite achievement would be commensurate with the infinite series of changes of which it is supposed to be the outcome? This would be a mere assumption. No reason can be given for it, and it is not self-evident. Indeed the analogy of infinite numerical series would suggest the contrary position. Consider, for example, the series . . . $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4, . . . It has no first term, yet the terms succeed one another in accordance with a definite rule. Now, let each of the terms regarded simply as an ordinal number, represent one of the infinite series of changes, and let the magnitude of each number stand for the value achieved at the date of the change corresponding to it. As the series continues the value is constantly increasing; but, although the series is infinite, the magnitude of the current term always remains finite. There is then no good reason for the assertion that whatever value has, as a matter of fact, been attained at any specified stage of world-history is not commensurate with an infinite series of changes.

A final word may be necessary to prevent misunderstanding. I am not arguing that there has been an infinite sequence of events. For all I know there may have been an absolute beginning. To be sure, such a beginning would be without rational explanation; but the "law of sufficient reason" is only a convenient assumption, which may or may not be treated as having universal application. All that I am maintaining is that there is no logical necessity impelling us to postulate such an absolute beginning.

RAY H. DOTTERER.

The Pennsylvania State College.

REVIEWS AND NOTES

Erkenntnis und wissenschaftliches Verhalten. By Arne Ness. Oslo: Norske Videnskaps-Akademi. Jacob Dybwad, 1936. Pp. 249.

In the course of the cycles occurring in the history of thought, the preferred center of organization for philosophy and the sciences, every now and then falls to psychology. Such a phase appears in Ness' book on *Cognition and Scientific Behavior*, in a new and modernistic version which seems to be more promising than the "psychologism" of the late nineteenth century. Whereas, at those times the emphasis was directed toward the subjectivistic "phenomenological" aspect, Ness, on the contrary, has sought to raise behaviorism, or, more generally, objective psychology, to the fundamental discipline. In many respects Ness appears to be influenced by American Pragamatism, the Logical Positivism of the Viennese Circle, and the Operationalism of Bridgman. In Ness' opinion epistemology and classical logic, as well as the dualistic "consciousness-psychology" of cognition, with all their mental acts, contents and meanings, should be replaced by an "objective psychological" description of the modes of scientific behavior observed and measured by the usual everyday methods. However, not concrete behavior, with all of its observable traits included, enters into the intersubjective definition of the scientific operations, but rather a selection of certain "relevant" abstract traits. The "behavioristics" of science becomes thus a highly formalized discipline dealing with the properties of gross behavior which are effective in producing a certain group of achievements. Ness follows the trend of "Purposive Behaviorism" in stressing the point of "macroscopic" observation as opposed to the methods of a molecular, microscopic behaviorism, represented by the traditions of physiologism. He adopts certain procedures of Tolman in redefining the traditional concepts of psychology in terms of "molar" behavior. He defines, for example, "erroneous judgment" as an "interrupted" pattern of behavior, i.e. one which does not arrive at its usual end. Achievement (*Leistung*) is made the central issue. The concept of cognition (*Erkenntnis*) thus becomes released from its usual entanglement with the restricted and isolated field of verbal symbolization, and

placed within the more general framework of the organism's whole life and activity.

The agreement in which Ness stands with the modern forms of behaviorism as represented by Tolman and by Lashley, as well as with the *Umweltforschung* of Uexküll, and with the "Psychology in Terms of Objects" of the present reviewer is treated at length in his book. It is the common—implicit or explicit—presupposition of all of these systems, to take the world of the physicist as an unquestioned frame of reference. This is correct, Ness admits, as long as one remains within the narrower field of strictly psychological questions, such as the opposition between estimation (naïve perception) and physical measurement in a comparative analysis of their respective achievements. The raising of the realm of the physical to the position of an absolute reality, however, would contain, according to Ness, an error similar to that which would be committed by ascribing to the human "behavioral field" (*Umwelt*) a higher dignity than to that of some other animal, e.g., the rat. Such an error would lead, and has led, to what Ness calls "maze epistemology" (*Labyrintherkenntnistheorie*). This error is committed in all cases in which the "features of the environment mastered" by an animal are considered as a part-selection from "reality," i.e. from the behavioral field of the physicist. It should be kept in mind, however, as Ness emphasizes, that physics too is relative in the sense that it has undergone a development of gradual differentiation from the naïve world of perception. Ness points out, by choosing as examples the development of the measurement of temperature and the determination of the distance of the sun from the earth, how "certain laws which Brunswik has found to hold for estimation (perception) seem to be valid for scientific behavior in general and therefore also for measurement." The measurements, too, vary, or oscillate about certain values. Furthermore, with reference to the way of formation of concepts, the older notion of "temperature" is "diffuse" in comparison with the present modified and redefined physical concept. "There is no reason to assume that this process of correction is ever going to stop."

One of the most peculiar trends in recent objective psychology and in science in general is the tendency to weaken the ties of traditional "absolute" distinctions. These distinctions are replaced by gradual conceptual transitions, as is done in many-valued logic or in the "object-critical" analysis of perceptual achievement. Ness mirrors and furthers this trend by questioning the absolute character of the antagonism between the empirical and the logical, and, within the latter, between

true and false. The origin of these concepts in, and their connection with rather specific behavioral situations is emphasized. The revolutionary keenness with which Ness deals with topics of this general nature is, even where it becomes impetuous, one of the most impressive features of this interesting and promising first publication.

EGON BRUNSWIK.

University of California.

TIME AND TIDE WILL WAIT

The Span of Life. By William Marias Malisoff. The J. B. Lippincott Co., Philadelphia, 1937. Pp. 339.

The readers of *Philosophy of Science* know what to expect when Dr. Malisoff writes a book. The present extraordinary volume—a study in optimism for a discouraged world—is no disappointment. Here we find an amazing knowledge of a vast range of facts, a facile imagination which plays upon the facts, a certain irreverence for hoary tradition, and a literary skill which enriches the language. If here and there the style is conversational and even jocose, this is relieved by statistical embellishments, and the whole is balanced off with a sober concern for an ethical program which contains elements of surprise and eloquence.

Dr. Malisoff's theme is an old one. The dream of youthification gives rise to endless myths of the phoenix rising from its ashes, elixirs of life, philosophers' stones, fountains of youth, and so on. The author covers the entire subject from its pseudo-scientific stage to the present vastly modern methods of research, and amply justifies his thesis—based in part on his own researches—that the "span of life" for man may well be prolonged to at least a hundred years. If life be invention, its greatest invention will be the discovery of the means to perpetuate itself. The staving off of death is the most fundamental of all considerations and its attainment calls for a Coöperative Research Foundation based on an "ethical decalogue of science" and a "unified science" program.

To the philosopher the most interesting ideas are likely to be those which define the outlines of the author's emerging philosophy, much of which seems to revolve around the pivotal suggestion that in the course of time certain traditional types of logic will disappear. In connection with the problem of the origin and nature of life (*mechanism vs. vitalism*) this idea is linked with the suggestion of Edgar A. Singer, Jr., who has shown that the "Democritean postulate," stated mechanistically, and the "Aristotelian principle," stated vitalistically, are not contradictories. Flying in the face of formal logic, and resorting to the technique of

symbolic logic, Professor Singer seeks to demonstrate that there are many "universes of discourse" and that classes defined to be contradictories within one universe need not be such within a second. Malisoff here states that Dr. Singer has made an important contribution to the development of a view "beyond mechanism and vitalism." Dr. Malisoff returns to this point later on, argues that we can talk about "possible" and "impossible" only within a given context (e.g., it is impossible to square the circle within the context of plane Euclidian geometry, but *not* impossible in other mathematical contexts), and thus concludes that the actual revolutions in science (accomplishing things declared to be impossible by the "inevitabilists") are a constant refutation of those pessimists who follow the "logic of despair." (In certain respects this thesis seems to resemble the "non-Aristotelian logic" of Brouwer when he holds that the law of excluded middle does not apply to infinite aggregates.) Malisoff states that with our finite set-ups the impossible can only mean: impossible in a given definite set-up, but nothing is generally impossible. Thus in the modern theory of the electron we find that *something may be both present and absent;* and in chemistry the demonstration by Carrel of the impossibility of rejuvenescence because *certain chemical reactions are irreversible* turns out to be correct only within a certain context and not true within the context, e.g., of Tolman's relativity thermodynamics. After all this it is easy to reiterate with Malisoff: TIME AND TIDE *WILL* WAIT!

These are some of the interesting highlights of a fascinating volume. If the treatise does not parallel in popularization the work of Eddington and Jeans, certainly it belongs to the best tradition of Alexis Carrel and J. B. S. Haldane. I can only conclude by saying: this is a *civilized* book —let's have more like it!

Oliver L. Reiser.

University of Pittsburgh.

BOOKS RECEIVED

An Introduction to Modern Thermodynamical Principles. By A. R. Ubbelohde. Oxford University Press, New York. 5/20.

The Nature and Meaning of Evil and Suffering. By C. J. Bond. H. K. Lewis & Co., Ltd. London. 1s.

Einführung in die Mathematische Logik. By Alfred Tarski. Julius Springer. Vienna. 7.50 RM.

Einstein's Theory of Relativity. By A. I. Katsh. Tel Aviv (In Hebrew).

Mach's Empirio-Pragmatism in Physical Science. By C. B. Weinberg. Albee Press, New York.

Determinismus und Indeterminismus in der Modernen Physik. By Ernst Cassirer. Göteborg, Wettergren & Kerbers Förlag.

Procedures and Metaphysics. By E. W. Strong, University of California Press. $2.50.

Theodor Gomperz, 1832–1912. By Heinrich Gomperz. Gerold & Co., Wien. Mkl. 50, S2.80.

An Introduction to the Philosophy of Science. By A. C. Benjamin. The Macmillan Co. $3.50. Pp. 469.

The Critique of Poor Reason. By Arnold H. Kamiat. Privately printed. $1.00.

Genetics and the Origin of Species. By T. Dobzhansky. Columbia University Press. $3.60. Pages xvi + 364.

The Physical Treatises of Pascal. Translated by I.H.B. and A.G.H. Spiers and Introduction by F. Barry. Columbia University Press. $3.25. Pages xxviii + 184.

Recent Theories of Sovereignty. By H. E. Cohen. The University of Chicago Press. $3.00. Pp. 169.

The Problem of the Individual. University of California Publications in Philosophy. Vol. 20. Pp. 1–206.

The Nature of Mind. University of California Publications in Philosophy. Vol. 19. Pp. 1–232.

ERRATA

E. Brunswik, Psychology as a Science of Objective Relations, *Philos. of Science 4*, No. 2, 1937, pp. 227–260

P. 228, line 16, read: "changes in spatial orientation"
In footnote, line 6 from the bottom, read:

"1936, 96, 467–495."

P. 229, in footnote 2, last sentence should read as follows:

"In fact, abandonment of the 'constancy-hypothesis' even clears the way for finding that other physical something—body size—which does stand in a (fairly good) one-to-one relationship to the reaction and thus represents the environmental term which gives proper sense and meaning to the establishment of the reaction."

P. 233, next to last line of text: read "nodal" instead of "modal"
Last line of text, read: "spatial and temporal gap"
P. 235, line 10, read: "type of object influencing the reaction"
Line 24, read: "likewise not the intensity"
P. 238, line 19, read: "at different distances, in different orientations"
First line of footnote, read: "B. F. Skinner"
Line 3 of footnote: close quotation marks after "end-terms"
P. 240, last line of footnote 15: read "datum" instead of "dictum"
P. 241, line 10, read: "*restrict ourselves to the subject's report as to equalities*"
P. 242, first line: read "wind" instead of "mind"
P. 244, last line of footnote 20, read:

"page 251 f."

P. 247, next to last line of text, read: "modes of evaluation"
P. 248, line 4, read: " 'confrontations' of"
P. 252, line 5 of part IV: read "world" instead of "word".